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The Control of Liver Fluke in Sheep and Cattle.

Discussion on Mr. Norris's Dublin Congress Paper.

Professor J. F. Craig, M.A., Principal, Veterinary College of Ireland, Dublin, presided at the discussion on the paper on "The Control of Liver Fluke Disease in Sheep and Cattle," prepared by Mr. James H. Norris, Director, Veterinary Research Laboratory, Thorndale, Drumcundra, Dublin.

Mr. Norris said that before the discussion took place he wished to make certain corrections in his paper as printed. On page 123 the two last lines should read: "The doses employed varied from single doses of one drachm, 4 c.c., 5 c.c., and 6 c.c." etc. The opening of the last paragraph on page 125 should read: "The results show that 45 c.c. carbon tetrachloride, given in three daily doses," etc. He stated on page 160: "Walton found a 1 per cent. of copper sulphate gave satisfactory results, but for rough pasture he recommends a 2 per cent. solution, applying up to 120 gallons to the acre." He (Mr. Norris) found out afterwards that Walton employed as many as 140 gallons in rough pasturage where there was a good growth of grass.

Mr. Robert F. Montgomerie, B.Sc., Veterinary Advisory Officer, University College of North Wales, who opened the discussion, said: It would be an unintentional omission on my part if, at the outset of my remarks, I did not express my appreciation of the honour the Provisional Committee have done me in inviting me to open the discussion on the paper which we now have before us.

In the opening paragraph of this paper it is stated that the severe attack of 1924-25 gave a fresh stimulus to research work on liver rot. I am sure that, having read this article, you will agree that this stimulus was accepted to good purpose by Mr. Norris. Fifty pages of solid work have been presented before you to-day, less than two years from a time when, I suggest, perhaps only ten such pages could have been produced and then only by extensive reference to foreign literature. It is evident that a great deal of hard thought and hard work has been brought to bear upon the subject. I am well able to appreciate the value of the paper we are about to discuss and the work it represents. Mr. Norris deserves the congratulations of this meeting, and I take this opportunity to offer mine. (Applause.)

I propose to take you over the paper and to draw attention to certain parts which, I believe, are worthy of particular notice and of comment.

I think it is unfortunate that in a paper of this kind the common liver fluke (*Fasciola hepatica*) should be declared the cause of liver rot, without any mention of *Dicrocoelium dendriticum*. It is true that Cameron, in the excellent paper he presented to our Congress last year, declared *Fasciola* the only important fluke of these islands, and that it alone has been found during my stay in North Wales. Yet, less than a year ago the late Chief Veterinary Officer spoke to me of an outbreak of liver rot within Great Britain in which the infesting flukes were *Dicrocoelia*. This is of particular importance, since it is believed that the latter species is less easily assailed by drugs, or, at least, by extract of male fern.

The review of the life history of the fluke is worthy of some consideration, for certain features of it are of primary importance when considering the measures to be taken for the control of liver rot. As is indicated, perhaps not in so many words, we should hesitate before we accept material which has been handed down from one generation of text books to the next. The observations on the "incubation period" of ova are more than interesting. It is something new to me to hear that ova taken from the bile ducts hatch more readily than those obtained from fæces. I wonder if Mr. Norris can offer us any explanation of this feature.

Mr. Norris says the cercaria encyst on blades of grass or aquatic plants. A percentage, perhaps as small as six, encyst on the surface of the water. It is important to bear this in mind. These free cysts may be carried long distances to contaminate the drinking water of sheep or cattle grazing "dry" fields, or even cattle which are housed. I know of more than one outbreak of liver rot among cattle in which there was very strong evidence, although not complete proof, that the source of infestation was trough water drawn from a snail-infested pond some distance away.

At the top of page 113 we read: "Sheep and cattle become infested during the late summer and autumn periods." This is, of course, text book teaching, and it is true that these are the chief periods. Infestation may, however, occur at a much later season. During the winter of 1924-25 many sheep in North Wales picked up encysted flukes after the passing of the old year. Following a very open wet winter, sheep may be infested even during the very early spring months. One of my friends in North Wales advised a client that, about the middle of January, he could safely turn a flock of ewes on to low-lying damp land which had been kept free of stock since early autumn. The meadow was known to be "dangerous." That flock became infested, beyond all doubt, after they were turned to that meadow.

It may be well to emphasise that stage of the life history of the fluke immediately following the ingestion of the encysted form. Escaping about two or three hours after ingestion, the young fluke bores through the wall of the intestine, wanders for a period within the peritoneal cavity (these wanderings

may occupy from four to fourteen days), is attracted to the liver, and bores through the capsule into the substance of that organ to migrate to the bile ducts. This phase is important in view of the inability of the drugs at present used to assail the parasite at that stage, and the certain difficulty in the diagnosis of acute cases of liver rot.

I do not propose to occupy time in discussing the symptomatology and pathology of liver rot, as they occupy a less important sphere in the control of the disease. These matters are well set out by the writer of the paper.

I must confess, however, that I was disappointed to find that a paragraph had not been devoted to the diagnosis of liver rot in its various phases. It is true that here and there throughout the paper information which will aid diagnosis is given. It might well have been summarised in a special short paragraph.

The first measure of control which has been considered is that which sets out to cure sheep affected with liver rot; to destroy the parasites within the mammal host; to prevent the scattering of ova containing fæces over the pastures.

Mr. Norris has given primary places, I think very rightly, to his experiments with carbon tetrachloride. At Bangor, during the past winter, we investigated the toxicity of this drug to sheep infested with the common liver fluke and its use in the treatment and control of liver rot. A record of the investigations was published in the June issue of the *Journal of Comparative Pathology and Therapeutics*. The two investigations have been independent.

Perhaps I may be permitted to point out that the name of the drug is composed of two words which are not linked by a hyphen. This may easily have been a printer's error, but it might well be corrected now.

Mr. Norris has emphasised the necessity of obtaining the drug pure. During the Bangor investigation it was shown that the consistently satisfactory results given by pure carbon tetrachloride could not be obtained when the commercial product was used. In that part of the Bangor article which dealt with commercial carbon tetrachloride I said that its chief impurity was carbon bisulphide. The work to which Mr. Norris has referred had not appeared when it was ready for publication, and before I was aware that I had mis-stated the facts the article was with the printers.

The subjects of the Bangor toxicity experiments were affected with liver rot; those used in Mr. Norris's experiments were not. Believing, as Mr. Norris also does, that the drug is best administered in capsule, we confined our attention to the toxicity of the pure drug when so administered and to the danger associated with its liberation in the mouth of sheep by accidental breakage of a capsule.

Our affected subjects perfectly tolerated larger doses than those given in the experiments recorded in this paper. For example, a seriously affected sheep weighing 58 lbs. received 35 c.c., and another, only 45 lbs. in weight, 50 c.c. In neither case were toxic symptoms observed. It is, therefore, evident that these affected sheep showed a greater tolerance than did the healthy sheep used in the Dublin experiments. Our experiments were not pushed so far as to allow the presentation of *data* regarding the actual toxic dose, but indicated a considerable tolerance, as large as 1·1 c.c. per lb. body weight. It was also found that the breakage of a capsule containing 5 c.c. produced certain undesirable symptoms but need not occasion alarm. Capsules containing smaller amounts when broken in the mouth of sheep gave rise to only transient effects.

The method of ova enumeration detailed is one which, I should say, the practitioner might well use to assist diagnosis, to control the effectiveness of treatment, or to obtain an indication of a necessity for re-dosage. As a method for use in scientific investigation, I do think that it might well be tied down to something more accurate than is indicated by the expressions "about two grammes" and "a small quantity of sediment was evenly spread over a measured space (one inch square)." At a later stage in his paper Mr. Norris appears to suggest that the method in his own hands has a very limited value.

I think I may pass over the detail of the twelve experiments with carbon tetrachloride on infested sheep to the discussion on them. Mr. Norris's work leads him to say that a single dose of 6 to 10 c.c. would probably be effective and safe for sheep of any age. The Bangor experiments show that a much smaller dose has an efficiency equal to that claimed for these doses. In our first experiment a single dose of 7 c.c. (in capsule) was found efficient, and thereafter, in a series of experiments, the dose tested in each being smaller than that of the previous one, the administration was efficient. The minimum dose tested was ·5 c.c. Continuing this series, a single dose of 1 c.c. in linseed oil was shown to be efficient, and this dose, given in capsule, without preparation by starvation for a period, destroyed all the mature flukes infesting the subjects of the two tests. In one of the latter we obtained our first observation, which indicated that the administration did not destroy immature parasites.

We selected a *single* dose of 1 c.c. in capsule, for field trial. This dose was selected since a capsule containing that amount is of a convenient size, and because it appeared to be far removed from the probable toxic and probable sub-efficient doses.

Fairly extensive trial (in all, 172 sheep seriously affected with liver rot were used) bore out the evidence obtained during the preliminary work. We found the dose tested efficient, so far as adult flukes were concerned, for subjects weighing from 38 to 140 lbs. The latter weight was that of the heaviest-affected sheep available. It remains to be seen whether the range of efficiency of this small dose may be extended to cover sheep of all the weights which may be met in ovine practice.

It was shown that preliminary starvation was not a necessary adjunct to treatment with carbon tetrachloride, and that the dose mentioned could safely be employed in the treatment of ewes almost at the close of their gestation period or with lamb at foot.

The field trials gave further evidence of the inability of the administration to destroy immature flukes and showed that those parasites which escape may mature to the egg-laying stage as soon as the fourth week after dosage. It remains to be shown whether larger doses of carbon tetrachloride will destroy the fluke at an earlier stage in its development. From the point of view of control, and for the treatment of the common types of liver rot, dosage with 1 c.c. is efficient.

Perhaps I may say, really as an aside, that I am fully aware, from the work of Hall and, in addition, some Bangor experiments, that a dose of 1 c.c. of carbon tetrachloride is not efficient against alimentary parasites. In cases in which there is a mixed infestation the dose mentioned would require to be increased, probably to 5 c.c. At the moment we are discussing the control of liver rot.

I am particularly interested in the record of the three experiments with cattle. At Bangor we have been handicapped by lack of funds, and have been unable to extend our work to the treatment of liver rot in cattle. Let me tell you what happened in an attempt to make some observations on affected cattle. A practitioner friend very kindly suggested that he might try out material on some cattle belonging to clients, which cattle we had shown to be affected with liver rot. Having a knowledge of Hall's work on the toxicity of carbon tetrachloride towards cattle, I felt quite " safe " in giving a dose of 10 c.c., to be administered in about half a pint of linseed oil. My friend asked for two more doses. He had three in all. You may guess that the following note, received two days later, gave me a shock, " Better quit that poison. The three animals which had it died within a few hours." It may be noted that we had previously given this same dose in linseed oil to a sheep which, while it did show some toxic symptoms, was not seriously affected. I think it is evident that cattle possess a much smaller tolerance for this drug than do sheep, and it would appear that, until much more work has been done on the subject, carbon tetrachloride cannot be recommended in the treatment of parasitic conditions in cattle.

It may be mentioned here that a small series of experiments conducted at Bangor showed that tetrachlorethylene, another drug introduced as an anthelmintic by Hall, did not possess an anthelmintic action towards the liver fluke comparable with that of carbon tetrachloride. This finding is of interest, but we need not stop to discuss it now.

I have felt it advisable to consider at such length the use of carbon tetrachloride that I must now hurry through the part of the paper which deals with male fern. I may be excused if I deal only briefly with this portion of the paper, for I feel that treatment with male fern, so far as ovine patients are concerned, is now almost a thing of the past.

Throughout this paper we find frequent references to the " strength " of the drug, and one might gain the impression that the chemistry of male fern, together with the value of its various active constituents was, at least, fairly well known. After the study of quite a number of works on this subject, I am entirely at sea. I read of at least five active constituents, and that no reliable process is known by which each of these, individually or in total amount, can be determined. I think the B.P. standard is wrongly given in this paper and should read " the sample must contain not less than 20 per cent. of filicin." A recent authoritative article says that the toxicity of fluid extract has been shown by a series of experiments to bear no relation either to the amount of crude filicin or of filicic acid present. I have not yet seen the experiments which prove that the filicin content is indicative of the therapeutic action of the drug. We gain the impression from this paper that Mr. Norris believes it to be a good index.

Experimental observations have not yet tied the efficient dose of liquid extract of male fern down to narrower limits than those indicated in this paper.

Since a sentence appearing on page 127 is in italics, we must consider it particularly and make special comment upon it. I am with Mr. Norris in saying that care should be exercised in the dosage of animals which are very weak, but I suggest he would have made a better recommendation had he advised that " When the subject is very weak the dose recommended above should be reduced to one half and administered on four successive mornings instead of on two." I do not think there is any real value in the recommendation contained in the second phrase of that sentence. It reads, " and if signs of distress follow the first dose, the second should be withheld for some days." Mr. Norris would have us believe that when we are administering doses of male fern, close to the limits of toxicity, that is doses believed to be sub-toxic but which prove to be *just* toxic, we are likely to find the first of the two doses give rise to " signs of distress " on, or before, the morning following its administration. That is not our experience. During our experiments, when we were administering doses which turned out to be just within the limits of toxicity, " signs of distress " were not apparent on the morning following the first dose, that is, when the administration of the second was due. At the earliest, toxic symptoms appeared during the early afternoon of the day on which the second dose was given, each dose having been administered in the morning. If this experience be the general one, what use is there in this recommendation ? Mr. Norris has also said that the second dose should be withheld for some days. I take it he would have us believe that the administration of these two doses at such an interval would be efficient treatment. I suggest that, if they would, the principle of dosage on successive mornings falls to the ground, and the recommendation should be that an interval of " some days " elapse between the double doses recommended. I do, for my part, recommend dosage on successive days.

In our experience, we obtained information, perhaps rather scant, which lead us to believe that the efficiency of the extract, when directed against the liver fluke, may be hampered by its administration in capsules. Mr. Norris says that it may be given in capsule but does

not indicate whether he has tested the effect of administration in such a manner on the efficiency of the drug. I certainly prefer warm milk as a vehicle, although it is not an ideal one, and certain precautions must be observed to ensure proper "emulsification."

From the *data* given in Table I Mr. Norris suggests "that the drug has a selective action on the fluke, for the percentage of filicin found in the bile of sheep which harboured none, or few, parasites is in many cases noticeably higher than in those where a considerable number of flukes had been killed. May I suggest a different deduction from the figures given? Could it not well be that the impairment of the liver function, through fluke infestation, brought about a decreased excretion of male fern in the bile? This functional impairment is, I suggest, the reason why infested sheep do not tolerate male fern so well as healthy animals. That has been shown by certain experiments made during the past winter in North Wales.

In the next paragraph of his paper Mr. Norris shows that the percentage of filicin increases with the age of the extract. It is accepted that freshly-prepared fluid extract is much more toxic than old material. We must, then, wonder what the significance of a knowledge of the filicin content of a sample really is. Some work, as yet unpublished, shows that extract of some age is less toxic towards sheep than is freshly prepared extract. I believe that the efficiency of fluid extract, when directed against the liver fluke, is not decreased by age. These features suggested some time ago an important line of work which might lead to the recommendation that old extract is much more safe and at least as efficient, as new material. I hope that the opportunity to continue the work on the effect of age on extract of male fern will be given, for I think it is an important general point although, now, not of first importance in connection with liver rot.

Mr. Norris has drawn attention to the variation of samples of male fern. Recently we, at Bangor, conducted a simple comparative test of the toxicity and efficiency of material obtained from four British drug houses of repute. No significant difference was found. I would say, then, that the age of the extract may be of first importance from the clinical standpoint. Confirmation of this view must await the appearance of the results of further work, but the *data* I have is, to me, very suggestive.

We have before us results which show that male fern and preparations of that drug are efficient against the adult liver fluke. We have evidence that at least certain parasites which have not matured to the egg-laying stage escape. We see how, in certain cases, about six weeks after treatment, ova of the liver fluke re-appear in the fæces. This certainly indicates that immature flukes were present at the time of treatment, and that they escaped destruction. Apart from the fact that opportunity for infestation did not exist after the time of treatment, we must accept this contention that the flukes were present at the time of treatment, for, from my own observations, it would appear that a period greater than six weeks elapses between the picking up of the encysted young fluke and its maturity to the egg-laying stage.

To the general discussion on treatment I will simply add that after the observations of 1925-26 I am ready to accept carbon tetrachloride as *the* anthelmintic for the treatment and control of liver rot of sheep. So far as cattle are concerned, I believe that further work must be awaited before we can say that the problem of the treatment of the disease in these animals is on sure ground. Mr. Norris's work has very materially advanced our knowledge of that problem. I believe that the advantages of carbon tetrachloride over male fern could well be put more emphatically. It provides us with a mode of treatment which may fearlessly, from a toxicity point of view, advisedly, from an efficiency point of view, and economically, from a costings point of view, be applied in the mass treatment of liver rot in sheep. That is what we wish.

It is true that no really efficient method of treating so-called "acute cases" of liver rot has been found. These are cases in which the whole, or the major portion of the infestation consists of flukes which have not yet reached maturity. I would point out that this is not of high importance from the control aspect, for these flukes have not yet reached the stage at which they are able to propagate their species and that dosage with either of the drugs recommended at monthly intervals would prevent an infested animal from distributing ova-laden fæces.

That method of control which relies on the destruction of the intermediate host has been allotted only a small portion of the paper.

Efficient drainage is certainly one of the first lines of attack on these fresh water snails. Someone has said that many men would spend the price of their farm were they to drain it properly. Many places offer very knotty drainage problems. By cleaning out ditches and preventing them flooding adjacent land much can be done. One type of danger spot is the vicinity of a partially-blocked ditch whose sides have been broken down, and whose water flows over the adjoining land.

Walton, in a later issue of the *Journal of the Ministry of Agriculture* than that quoted in the paper, namely, November, 1925, has added further to our knowledge of the use of copper sulphate. He showed that this salt may be effectively used as a spray at the strengths of 2, 1 and ½ per cent., as a dust with four kaolin or broadcast mixed with four or eight parts of sand. I leave you to consult the article I mention if you wish further information, but I would express my belief that the application of copper sulphate should be much more widely used than it is at the present time.

In general conclusion, I would say that I take the Bangor investigation as establishing the efficiency of dosage with 1 c.c. of carbon tetrachloride in the destruction of flukes which have reached the egg-laying stage, at least so far as sheep of weights up to and including 140 lbs. are involved.

Perhaps you will consider infestation of cattle and rabbits less important than that of sheep from the control point of view. If you do, I suggest that we now

possess modes of preventing the liver fluke propagating its species so efficient, when properly applied, so safe and so cheap that the time has come when the proper control of liver rot is within reach.

I hope we will do all we can to discourage the practice, probably originated by the use of relatively expensive drugs, of dosing individual sheep of a flock as they become clinical cases of liver rot. It is a wasteful practice. It should be our endeavour to make plain the advantages of dosing, at the very least once, every member of every flock among which evidence of the existence of liver rot has been obtained. That I regard as the minimum extent to which dosage of sheep should be employed to control the ravages of liver rot.

On those farms which regularly or periodically suffer loss from liver rot, we must aim at something higher. Our minimum here should be dosage as a "preventative" at least once *every* year; probably during the earlier part of December. Our ideal certainly is dosage at monthly intervals, perhaps beginning in October and ending in April. May we have advanced to this stage, at least on many farms, before another outbreak comes upon these Islands. There can be no doubt but that those who adopt such methods will reap a reward exceeding their outlay many times.

Before we close this discussion we might well recall the work of Dr. Maurice C. Hall. Without mention of his many other admirable activities, the introduction of carbon tetrachloride as an anthelmintic deserves our gratitude. We may well hope that at an early date the duties of that high office to which he has been called may spare him to visit a congress of the N.V.M.A.

I have to thank you, Mr. President and gentlemen, for the very patient hearing you have given me. I hope you appreciate the difficulty of a worker actively engaged upon this subject in discussing it. Someone may think that they have detected a note of egotism in some of my remarks. Please believe me that, if you felt anything of that nature creeping in, it was unintentional or, at the very least, unavoidable. If some of these remarks appear destructive, I hope they have disappeared in some constructive effort. (Applause.)

Mr. P. J. Howard (Ennis) said he was sure they were all very grateful to Mr. Norris for the work that he had done in connection with this matter, which was a very important one for Ireland and for places across the water as well; and they were particularly grateful to Mr. Montgomerie and interested in the account which he had given of his labours in connection with the same subject. He regretted that neither Mr. Norris nor Mr. Montgomerie had been able to find a means by which they could put new livers into sheep or cattle. (Laughter). He mentioned that in order to emphasise the view that their efforts in connection with the control of liver fluke and its ravages should be directed, as far as possible, to prevention, because he was satisfied from his experience—and it had been a fairly wide one in the last few years—that where either sheep or cattle were really badly affected it was a waste of time to keep them alive, except in the case of ewes in lamb, when they could get the lambs. They would kill the flukes, exterminate them from a particular animal and keep the animal alive for a considerable time with the aid of food nourishment and surroundings, but from an economic point of view the effort would not be worth what would be spent on it. His experience was that six or nine months or a year might elapse before affected animals would die off, and then it would be found that they no longer had livers—the organs had become something else. That was why he had been anxious to hear from Mr. Norris or Mr. Montgomerie whether they had been able to find some means of reviving the liver.

They must not run away with the idea that every land that was flooded would be affected with fluke. It was not so. There were many places that were very often flooded and yet fluke contamination was never seen there. So that there was something in the suggestion that they must have the presence of infected snails, to lead to contamination. He did not think there was any doubt that, by means of drinking water or water flowing along from infected places, the material was conveyed and might be the means of introducing the parasite. There was also the fact that in places to which ducks and geese and wild fowl had access they would considerably lessen the ravages of the snail.

Mr. G. Elmes (St. Albans) said that in the examination of fæces for the ova of the liver fluke it would be fund that a great peculiarity was that the ova would not float in normal saline. The explanation of that was very interesting. It had been a great mystery to him until he got enlightenment from Professor Leiper, whose explanation was that in the capsule of the ova there are two small holes near the top, and that this fact prevents the ova from rising.

Mr. A. W. Noel Pillers (Liverpool) said that as a profession they ought to be extremely grateful for the advances that were being made in research work with a view to dealing with conditions such as those arising from the ravages of liver fluke; and it was very satisfactory that suitable men were available to do this work. Without being a sentimentalist or without being extremely interested in birds, he did think that the National Veterinary Medical Association might have some association with the Society for the Protection of Birds so as to help in the protection of those birds that ate snails and so on. (Hear, hear.) As an example, one could not help feeling what a wonderfully valuable bird the lapwing was in that respect. Anything they could do to stimulate the protection of that bird—and it was not protected as it should be—would be of great value to agriculture.

With regard to the remarks made about the lanceolate fluke (*Dicrocœlium dendriticum*), he had met with it three times—once in a lamb killed in Liverpool; once in a mixed infection with *F. hepatica* from sheep probably of Scotch origin; and Mr. P. Brain, M.R.C.V.S., of Mr. Trotter's staff, had sent him specimens. There was, therefore, definite evidence

that there was at least one area in Scotland from which infected animals might disseminate the lanceolate fluke.

It would be valuable if veterinary workers recorded their findings when they came across this species. The common fluke in rabbits was, as far as he was aware, extremely rare. When there was an infection of parasitic disease anywhere the first animal that was blamed was nearly always the rabbit. He had found fluke in rabbits in a district in which no sheep were affected.

With regard to the important remarks made by one of the speakers with reference to the possibility of replacing the liver function, the destruction of the parasite in the animal was quite a different thing from the cure of the lesion which had been left behind. Quite distinct treatment was required for the destruction of the parasite and the cure of the lesions which it had produced. One of the speakers had emphasised the importance of prevention. He (Mr. Pillers) thought it should be absolutely laid down that treatment of all animals was the thing necessary in the case of fluke before they could get to prevention. (Hear, hear.)

Mr. J. P. RICE, B.Sc., (Head of Animal Diseases Division, Ministry of Agriculture, Belfast) said he would like to congratulate Mr. Norris very heartily on the able paper that he had placed before the meeting. He would like Mr. Norris to tell them the points on which the ordinary practitioner could rely for the diagnosis or recognition of liver fluke eggs. In the laboratory in the North of Ireland, of which he had charge, they received a very large number of samples of fæces, and he thought much of the work now undertaken in the laboratory might be undertaken by the practitioner, to whom a description by Mr. Norris would be very helpful. In Northern Ireland and in Southern Ireland and elsewhere the symptoms exhibited by affected animals had often been so meagre and indefinite that one had to resort to the examination of fæces. Mr. Pillers had asked a question of Mr. Norris regarding fluke in rabbits. Might he say that in County Fermanagh, Mr. Hart reminded him, rabbits died in large numbers in October, 1924, and flukes were recovered from them.

Professor DE BLIECK (Utrecht) congratulated Mr. Norris on his splendid work. There was a great deal of discussion on the Continent last year about these remedies against fluke; and he would like to say something about the remedy serapis, which was made in Germany in the neighbourhood of Munich. It had been tested by Professor Ernst, who was a very trustworthy authority, and he had given the results in a publication which he had issued about a year ago. Professor Ernst had very good results with serapis in the case of sheep and cattle as a remedy against fluke. In Holland they used distol, which they found to be a very good remedy against liver fluke, but though it was not as expensive as danistol in these countries, still it was very expensive, and they looked for another remedy which would be cheaper. They had thought that they had found it in serapis, but in Holland they had found serapis very toxic for cattle, the reason being that there was a great percentage of carbon tetrachloride in it. Recent investigations in Germany corresponded with the results of their investigations in Holland. Many cases of deaths among cattle resulted from its use, and they had ceased to administer it to cattle. They did not now use it in Holland; they had fallen back on distol. They must be very careful about giving carbon tetrachloride to cattle. If they had good results in the laboratory, it did not follow that they would have the same good results in the field in regard to the toxicity.

As regards the question of treatment or prevention, treatment was prevention, because by treating the animals and killing the worms they broke off the cycle of evolution of the parasite and so prevented the disease.

The CHAIRMAN said he had a few comments to make arising out of the remarks made by Mr. Montgomerie. With regard to the lanceolate fluke, they had only met with two specimens of this species in Ireland in infected livers during the past twenty years, so that the probability was that the species was of very minor importance. With regard to the cercaria encysting on the surface of the water, it could not be said that the cercaria cysts were deposited on the water. It was more probable that they were washed off the grass and carried away by the water which flowed over it. Concerning the question of periodicity, certainly the evidence that they had in Ireland went to show that, as an almost invariable rule, the gross infestation only occurred in the late Autumn.

THE REPLY.

Mr. NORRIS, replying, thanked Mr. Montgomerie and the other gentlemen who had taken part in the discussion for the very kind manner in which they had treated his attempt to furnish a paper for discussion. As the time for reply was very short, he would go over the ground very rapidly. Mr. Montgomerie's first point was as to the varieties of fluke. In his experience—and it had been fairly considerable within the last two years—he had seen no other variety than the common liver fluke; and he did not think that any other variety was at all important as far as Ireland was concerned. He was interested to hear that in a few outbreaks of liver rot in Great Britain the *Dicrocoelium dentriticum* was the parasite present. He should have mentioned the possibility of this parasite causing the disease, as different methods would be necessary for its control. The next point was the hatching of the ova. Mr. Montgomerie wished for a reply as to why eggs taken from the bile ducts hatched more rapidly than those obtained from fæces. That was a point in which he could not definitely satisfy Mr. Montgomerie's desire. It was a fact that when they took bile containing eggs and placed them in a test tube of clean water they would hatch almost uniformly more rapidly than eggs in fæces. Possibly the medium of clean water gave quicker results than water contaminated with fæcal matter.

Mr. Montgomerie made another point which, perhaps, was important, if Mr. Montgomerie was correct in it, and that was that about 6 per cent. of the cercaria might encyst on the surface of water. Professor Craig had touched on that point, and he (Mr. Norris) was inclined to agree with Professor Craig. He thought there were cases in which cercaria had been washed down in streams and had caused infection in areas where snails did not exist. Whether these encysted on water or were washed off he did not know. He believed they were washed off.

The next point was as to the period of infestation. Infestation, as he (Mr. Norris) had pointed out in his paper, might go on the whole year round. He was interested to know that Mr. Montgomerie had definite evidence that it might take place as late as the month of January in some instances. A great deal would depend upon climatic conditions during the autumn and the winter months. Mr. Montgomerie referred to another point—he did not know whether he was speaking as the result of his own work or relying on some other authority—and that was that when a fluke passed through the wall of the intestine into the peritoneum it might remain there from four to fourteen days. In his (Mr. Norris's) opinion that was a phase in the life history of the parasite on which further research was required. Another important point was : how long did it take from infestation until the fluke reached maturity ? That was a point that had never been very fully worked out, and the time was ripe for further research upon it.

Mr. Montgomerie took him to task for leaving out a definite paragraph on the question of diagnosis, and he thought Mr.Montgomerie was justified. He should have had such a paragraph. At the same time, he thought that any person reading through his paper and having ordinary experience of diseases of sheep would have no difficulty whatever in arriving at a correct diagnosis. There would be a difficulty, he was sure, in arriving at a correct diagnosis on clinical evidence alone. With regard to the method of identifying the eggs, any person who had any experience in examining fæces for worm eggs would have no difficulty in diagnosing fluke eggs. They were peculiar in shape, size, and colour. They varied from 130 to 145 microns long by 70 to 90 microns wide, were yellowish brown in colour and distinctly egg-shaped.

With regard to carbon tetrachloride, he would like to congratulate Mr. Montgomerie on the work which he had done in connection with it in Wales. He was rather envious of the organisation between Mr. Montgomerie and the farmers. Apparently the farmers in his district could supply Mr. Montgomerie with a number of infested sheep for experimental purposes whenever he required them. That was a happy condition of things which he would like to see existing in every place, and it was the only condition under which the relations between the sheep owner and the laboratory would give good results.

As regarded the question of dosage, he was quite willing to accept a dose of 1 c.c. carbon tetrachloride as being effective as far as the evidence went, but he would like to qualify that by the statement that more experience was required. He did not think it would be injurious to increase the dose to a point which would leave no doubt as to the effect. He (Mr. Norris) had experience of only one case in which the drug had failed to kill the immature fluke. Mr. Montgomerie apparently had several such cases. Mr. Montgomerie had stated that the second crop of eggs might appear in the fæces of treated animals in the fourth week after treatment. That, in his (Mr. Norris's) opinion, was rather early. He thought they might put it at five or six weeks, as far as his experience went. It was the fifth or sixth week before the eggs appeared in any number. In order to deal with the second infestation they were safe in dosing in about a month.

He (Mr. Norris) had experience of a good many samples of so-called B.P. extract of male fern, and there did not seem to be any fixed standard as to the filicin content. Mr. Montgomerie was correct in saying that the B.P. standard should contain not less than 20 per cent. of filicin. He (Mr. Norris) did not hold that the chemical composition of a drug was a definite indication of its likely anthelmintic action. Such action was generally regarded as specific, and Mr. Montgomerie's results with the nearly-related carbon tetrachloride and tetra-chlorethylene against liver fluke confirmed this view. His (the essayist's) experience, however, had been that when the filicin content of male fern extract fell below 16 per cent. it did not give satisfactory results against liver fluke. He did not, however, wish to convey that the anthelmintic or toxic properties of male fern had been proved to be due to any one of the various constituents of the drug.

With regard to the treatment of cattle with carbon tetrachloride, there was no doubt that cattle did not show the tolerance for carbon tetrachloride that sheep did. They were only at the beginning of their experience as far as cattle were concerned, and there was a great field for further research. He did not believe that the three cattle to which Mr. Montgomerie referred had died from carbon tetrachloride poisoning. In his experiments, the animals were quite healthy for twelve or fourteen hours at least after administration. In these cases the animals died immediately. He thought they should look for some other cause of death. Of course, carbon tetrachloride was not a compound word

With regard to the method of ova enumeration used by him, he (Mr. Norris) did not claim accuracy, although the method had practical value. The important point in the method was not the amount of fæces taken for the test, but the uniformity of the smear of washed sediment spread on the measured area of the slide. Ova were not uniformly mixed in fæces, and it was difficult to see how any method which did not deal expeditiously with a large quantity of fæces could be expected to give better results.

Mr. Montgomerie had brought him to task about a paragraph that he had italicised. His point was that if the first dose caused distress or illness, the second dose should not be administered the next day,

as doing so would often lead to a fatal result. In his experience, it was better to postpone the second dose. He did not mean to convey, as Mr. Montgomerie rather inferred, that the first dose was the important one as far as toxic effects were concerned. It was the second dose, of course, which was the more likely to produce toxic effects. The toxic effects were not shown immediately after administration, but were generally delayed for some hours, as he had pointed out in his paper.

One of the speakers had referred to prevention *versus* treatment. He strongly believed that the farmers should do everything possible to prevent the disease by ridding their land of snails through the operation of spraying, drainage, etc. He also firmly held that if stock owners dosed their cattle and sheep periodically during the winter and spring months, they would considerably lessen the output of fluke eggs, and in this way prevent the snails on their land becoming infected. In this respect the treatment of infested animals, of course, amounted to prevention.

With regard to the question of fluke infestation in rabbits, he had had a wide experience in different parts of Ireland during the severe outbreak of 1924-25, and in only one instance had it been brought to his notice that rabbits had been infested. He took it that rabbit infestation from fluke was comparatively rare. He had not mentioned anything as to the influence of farm ducks and wild fowl, but he quite agreed that they performed very valuable service by the destruction of snails. He thanked those present very much indeed.

The CHAIRMAN said they were very much indebted to Mr. Norris for the splendid paper he had presented, and also to the opener of the discussion, Mr. Montgomerie, who had also given them the benefit of his experience and experiments in connection with the disease. Some members of the profession who were not members of the Association were, he was glad to say, present, and he appealed to them to join the Association. He was sure that their experiences of the past few days would show them that it was to their advantage to do so. (Hear, hear.) Adding to its numbers would increase the importance of the Association, and he hoped that the Congress would be the means of increasing the membership considerably.

This concluded the business of the session.

Royal Sanitary Institute Congress.

VETERINARY INSPECTORS' CONFERENCE.

(*Continued from page* 815).

DISCUSSION ON PROFESSOR BUXTON'S PAPER.

The discussion on Professor Buxton's paper, "Some Notes upon the Double Intradermal Tuberculin Test in Cattle and the Potency of Tuberculin" (published in last week's *Veterinary Record*), was opened by Major G. W. DUNKIN (Superintendent, National Institute for Medical Research, Farm Laboratories, Mill Hill), in the following terms:—

It is a matter of universal regret that the gentleman originally asked to open this discussion is unable to be present. I refer to Captain S. R. Douglas, the Chairman of the Tuberculin Committee mentioned by Professor Buxton in his address. I am possibly experiencing these feelings more acutely than others in this room, for Captain Douglas' absence has placed me in a position which I find it difficult adequately to fill.

By way of introduction to the few remarks I propose to make on the subject of the double intradermal tuberculin test, it will not be out of place, perhaps, if I say what a real pleasure it has been to listen to the essayist this morning and to add how much we are indebted to Professor Buxton for bringing this matter before us. We could not have had the subject more lucidly explained than we have had and it would be almost impossible to have the fund of information from anyone more *au fait* with the subject than he is.

As some of you may know, it has been my privilege to work at this subject, in the early stages of the investigation, with Professor Buxton, and more lately without his assistance, and as a direct result of this study the outstanding features of the double intradermal test that have struck me most forcibly are its ease of application, its practical utility in the field (perhaps the most important feature), the comparative ease with which the results can be interpreted and, last but not least, the fact that while under the test cattle can be treated just as if nothing had happened—that it is unnecessary to shut them up in sheds and so run the risk of serious loss in milk supply.

As I have already stated, Professor Buxton has such a real grasp of the facts that it is almost impossible to add anything to what he has told us. I would like to lay added stress, however, on certain points which he has mentioned in his paper. We have heard, for example, that "the character of the swelling is of the utmost importance and palpation of the site of the injection at the same time as the measurements are made is absolutely necessary." I want to say, Sir, how thoroughly I agree with this assertion. To my mind, the character of the swelling is of infinitely more importance than the actual size, although for purposes of record measurements must be taken.

I am, like Professor Buxton, well aware that certain owners are averse to having the necks of their animals shaved. The reason for this objection is obscure unless it is that if their animals react to the test the appearance of a shaved area on the neck might suggest to intending purchasers that the animal had reacted and consequently the sale might be spoiled. If this is the reason, then I say that it is an added argument in favour of shaving.

The question of the potency of tuberculin has been mentioned by the essayist and by this I am much pleased, for, to my mind, the success of this test and possibly also the success of any other test must depend largely on the potency of the material used. In my experience the use of a tuberculin of low potency spells disaster from the word "go."

From time to time statements have been made by persons possessed of certain inside knowledge that the practice of doping cattle against the subcutaneous test is much

practised. Whether this be so or not I cannot say, but I can say that I have never yet succeeded in doping an animal against the double intradermal test. I have, to-day, a cow on the experimental farm, to which I am attached, which has been tested at least thirty times in the last two years (apart from odd potency tests and an occasional large doping dose subcutaneously or intravenously) and on no single occasion has this animal failed to react.

I have nothing more to say except again to congratulate Professor Buxton on the excellence of his paper. I am optimistic enough to think that as a result of its presentation the vast majority of doubts which may have existed in the minds of certain practitioners will now have largely vanished.

Mr. G. P. MALE (Reading): Everyone will agree with Professor Buxton when he says that "any modification in technique which affords results at least as satisfactory as those obtained by the older tests is worthy of consideration and if, in addition, there is a hope of eliminating only some of the difficulties which attend the application of every method of testing yet devised, that modification cannot be dismissed without scrupulous scientific investigation and prolonged careful trial and practice," and therefore we welcomed the Tuberculin Committee of the Medical Research Council, of whom Professor Buxton, with his usual modesty, omitted to tell you that he was a member.

Since this report, I have carried out, with the assistance of my staff, nearly a thousand tests by the method advised, and there is no doubt that in practice it is a most useful test.

Professor Buxton, I am glad to say, does not claim that it is superior to the subcutaneous test, although the Tuberculin Committee of the Medical Research Council condemned it in no half-hearted fashion as being unsatisfactory under the ordinary farm conditions. This was most unfortunate, as it has proved so useful for so many years in this country.

We have probably tested nearly twenty thousand animals by this method and have been able to obtain tubercle-free herds without any other assistance, and to shake the faith of owners and veterinary surgeons in this way has not helped in the elimination of the disease from our herds, which is the desire of all of us.

I hope Professor Buxton will not take any remarks I have to make as being in the nature of destructive criticism but I will describe it as I have found it in practice, in the hope that it will lead to some benefit.

As has been said, the intradermal test has been carried out for a number of years in this country and in the United States of America, where they claim to be able to handle from three to four hundred cattle per day, and whole States have been cleared of the disease by this method.

Mr. Walker reports that he used it in 1915 in Colorado and he has since that time used no other method, but as carried out at that time my experience with it was not very satisfactory, especially when combined with the subcutaneous and ophthalmic, where it was uncommon to get a reaction to all three tests in the same animal, and it only appeared to confuse the issue.

At that time, in this country at any rate, the injection was made in the sub-caudal fold, under the tail, and only one injection was given.

There is no doubt that the modifications suggested by the Committee have improved the method very greatly; with only one injection there was a great number of indefinite reactions, which have been eliminated to a great extent by making a second injection on the exact site of the first. Unfortunately, however, contrary to the claims of the Tuberculin Committee, there is still a number of doubtful reactors; in fact, in my experience, more than by the subcutaneous method. True, a typical reaction is easily read and very definite, but those animals that give atypical and doubtful reactions are a great source of worry to the practitioner.

In order to clear the herd of tuberculosis, it is advisable to exclude not only reactors, but doubtful animals, and where there is a temperature chart to read one can guage the reaction to a nicety, but where one has swellings which differ in nearly every animal it is a much more difficult process, so that we have to resort to a further test by another method.

I will give you a few examples of variations resulting from this method:—

A. One cow tested by the intradermal method: the fold of the skin increased from 7 m.m. to 18 m.m. in 72 hours; swelling, hot, painful and œdema marked. On *post-mortem* examination Johnë's disease very marked, but no tuberculosis lesions were found.

B. Two cows tested by the intradermal method: skin increased in size from 6 m.m. and 7 m.m. respectively to 24 m.m. and 30 m.m., the swelling was hard but there was *no pain* and no *œdema*; when re-tested by the subcutaneous method both reacted.

C. Five cows in a herd tested by the intradermal method showed very slight increases in the measurement of the skin, but the pain on manipulation was marked. At a re-test by the subcutaneous method two reacted and three passed. In the same herd, one bull that had passed four previous subcutaneous tests showed a marked and painful swelling at the end of forty-eight hours and, although given a second injection, the swelling had disappeared twenty-four hours afterwards, but it was still very painful. When re-tested by the subcutaneous method there was no reaction.

I have also noticed in many cases, although the swelling has been very slight and no œdema was present, the necks have been very painful, in herds that have been practically free from tuberculosis, and we have had every reason to believe that they were healthy, so that I do not attach much importance to signs of pain on manipulation.

The above results have not, of course, been verified by *post-mortem* examinations, but we have always made it a rule to make *post-mortem* examinations on any cows that may die and we have found no tuberculosis; presumably, therefore, the subcutaneous test was effective.

In animals killed under the Tuberculosis Order, where this test has been used the results have been quite satisfactory although, of course, here again those cows that have not given a definite reaction have not been slaughtered. In fairness, however, to the subcutaneous method, although I have made hundreds of *post-mortem* examinations on reactors to this test, I have always found the disease.

With regard to the technique, we find an elaborate preparation of the skin site to be unnecessary, the ordinary

razor blade appears to be more satisfactory than a safety one and we only find it necessary to shave a small area.

The method of injection is most important and it is not quite so easy as has been claimed; it requires great care and patience, especially in a nervous animal, and often such stand best if not controlled by the nose. Again, where iron neck yokes are used there is a good deal of difficulty.

It is important that the tuberculin should be injected into the exact spot, that is to say, the deeper layers of the derma, and several attempts may have to be made before the desired pea-like swelling can be felt.

I have found the most effective a small curved needle, which was recommended to me by Mr. Gofton, as it does not break so easily and the syringe can be attached with more facility.

Professor Buxton quotes Mantoux as saying that no immediate effect is produced in healthy cattle, but our experience is the same as Mantoux's co-worker Vallée, that in some animals a swelling appears immediately after injection and disappears before twenty-four to forty-eight hours have elapsed.

Professor Buxton and the Committee seem to place a good deal of importance on the actual measurement, as did the original workers, but I think that the character of the swelling is the more important, although even here, as I have demonstrated in previous examples, either the pain or œdema were sometimes absent.

I have noticed also that, contrary to one's expectations, a marked systemic reaction occurs in quite a proportion of cases. I would like to ask why this should be with such a small dose, which is not supposed to be absorbed into the general circulation, and whether this would inhibit the reaction to the subcutaneous test if applied afterwards.

We have noticed also that some reactors, even two months after the test, showed a hard thickening at the point of injection. I would like to ask if this is to be expected.

Summing up the advantages of this test, I quite agree:—

1. That it is a very great saving in labour.
2. It is more likely to be reliable in some herds, especially young animals that have not been tied up before, as in such cases the temperatures are likely to be variable.
3. That it can be applied at any time during the day and does not necessitate going late at night and early morning to take temperatures, which will prove a great boon to the veterinary profession and the owner.

The insistence by the Ministry of Health on the 6th hour temperature has done more to hinder the production of certified and tuberculin-tested milk than anything else and had it not been for some of us who were keenly interested in the production of these grades of pure milk, and who have carried out the tests at an unremunerative figure, there would have been little of this milk produced in this country to-day.

Although having tested many thousands of animals, I have never yet found a cow that reacted at the 6th hour, and failed to do so at the 9th, except in those animals that had been previously tested a short time before. Why then is it insisted on?

Fortunately, the intradermal test has now come along and has been recognised by the Ministry, and only just in time, as many veterinary surgeons and owners would soon have refused to carry out these irksome conditions.

The essayist gives in the latter part of his paper some extraordinary results which he has obtained by applying the double intradermal test on herds which had been previously tested by the subcutaneous method; these results are absolutely contrary to those obtained in my practice and I should have liked much more extensive details on many points before I cast any reflections on the subcutaneous method, especially as to the brand of tuberculin, the method of application, the interval of time between the tests, whether cows were bought and introduced into the herd between these intervals, whether these cows were isolated before being mixed with the others, the amount of original infection in the herd, whether isolation was carried out, the extent of pasture land which might have been heavily infected and consequently grazed over, and many other things.

As to the herd in which the calves were found to be tuberculous, the giving of tuberculous milk by one cow in a herd would be quite sufficient to infect all these animals, as I have proved many times.

I had a case only three weeks ago in which the milk from a non-tested herd was found to be tuberculous; on examination of the udders, one cow was found to have chronic mastitis but there were no lesions in the udders of the rest. Six weeks afterwards I again examined the udders and found a young, healthy-looking cow with a very small, hard lesion in one quarter, and examination of the milk showed it to contain large numbers of tubercle bacilli. On *post-mortem* examination we found tuberculosis of one quarter and in the supramammary glands and one small nodule in one of the mesenteric glands, the rest of the carcase being absolutely healthy. In another case, six calves were infected with tuberculosis after consuming tuberculous milk which was incompletely pasteurised during the period of a fortnight only.

It is well known that some cows only respond to the tuberculin test once or twice and then may fail to react on several subsequent tests, which cows, on *post-mortem* examination, have been found to have tuberculosis. If, therefore, the owner of the herd had bought in one of those cows and tested it without positive results, it might easily have infected a large proportion of the herd before the next test. In a paper that I read at the Sanitary Institute Congress at Exeter, many years ago, I gave details of a number of such cases.

It is also well known that some cows when extensively affected may fail to give a reaction.

I do not know whether Professor Buxton's researches will enable him to say at present whether the intradermal method will eliminate such cases, which, of course, are very rare.

The examples he gives of alternating the two tests show that the two tests were equally effective, in that, which ever method was applied first, the cow reacted subsequently to the other test.

Professor Buxton tries to show that the intradermal is more reliable than the subcutaneous in the instance he quotes, and where after the subcutaneous test was applied

the intradermal method showed a large proportion of reactors. I am glad to say this has not been our experience, hence my remark—to which he called attention and to which I still adhere—that I should have no complaint if the intradermal method was found to be as reliable in practice as the subcutaneous.

The time has been short since the new intradermal test has been in application, but I can give, at any rate, some examples :—

Herd No. 1. Consisting of 58 cows which had been tested many times by the subcutaneous method, when tested intradermally two proved doubtful reactors, both of which were re-tested in three months and passed.

Herd No. 2. Consisting of 80 cows ; 79 passed the intradermal test, one was doubtful, which when re-tested passed.

Herd No. 3. Of 68 cows, showed two doubtful reactors and one reactor which had never been tested before, these doubtful cases have not yet been re-tested.

Herd No. 4. 70 cows were in the process of being cleared, having had two previous subcutaneous tests ; five were doubtful, seven reacted, four of which had not been tested before. Of the doubtful animals, four passed and one failed at the intradermal test.

We have not yet had a chance to test many herds by the subcutaneous method after the intradermal, but they show so far that both tests appear to be equally efficacious ; my practise now is to use these tests alternately.

Among the disadvantages of the intradermal test is the difficulty of testing animals some distance from home, as it would mean staying three or four days before this was completed.

There are also objections when testing pedigree cattle for abroad. On the whole, however, I am satisfied that it is a reliable test and that it will prove a boon both to the practitioner and to the owner.

Mr. WILFRID BUCKLEY, C.B.E. : I have been asked to take part in this discussion, but I have prepared no paper. I am the least experienced among all of you, and Professor Buxton has covered the ground very extensively. But I have had the pleasure of being a member of the Tuberculin Committee, and no doubt I was asked because of some experience I had, and perhaps they thought it an advantage to have a layman present. I have made a few notes, and there are some comments I should like to make. My only comment on Professor Buxton's paper is that I should like to correct one statement that the work of the Tuberculin Committee originated through the request of the Minister of Health. I am aware that Sir Alfred Mond, while Minister, made that statement on one occasion through inadvertence, but it was not the Minister of Health. With regard to clipping, as a member of the Tuberculin Committee, I have attended a good deal of their work, and on my own farm I have upwards of 200 head of cattle, and have been testing regularly since 1907. With regard to clipping, I would like to make one small point that, through experience with the Tuberculin Committee, who carried out a great deal of work on my own farm, I found once that the thicknesses of the skin in the double intradermal tests were higher than those seen with the same result on the same cows with preceding tests. I found that the razor being used by the cowman had the effect of irritating the skin. I am not sure, from my own experience, that it is necessary or even advisable to have clipping. I would like to agree with Professor Buxton, and also to disagree to some extent, and to agree with Mr. Male in regard to the question of sensitiveness. I am not in a position to judge from personal experience of some of my cows whether the swellings in some of those cows were painful or not. Professor Buxton seemed to me to contradict himself in regard to the question of feeling the swelling. I think he mentioned swellings that increased from six or seven to 26 or 30 millimetres. I would like to suggest that in actual experience it is very difficult in certain cases to know whether a cow is suffering pain or not. I have had the help of one of the ablest of my friends in the veterinary profession to examine some of my cows, and we have been absolutely beaten once or twice in, say, 130 cows, to know whether the swelling was sensitive or not. I have also two animals that Mr. Tutt, senior, of Winchester, and myself could not believe reacted to the intradermal test. But as a result of the subcutaneous test I was not satisfied to keep the animals in the herd. They were slaughtered and found to contain tuberculous lesions. Those are the only two instances, of personal experience, where the interpretations that Mr. Tutt and myself put on the intradermal test led us astray in our diagnosis. That brings me to this point that, as owner of the herd, I have taken exactly the same position as Mr. Male mentioned. I am going to test by the subcutaneous test on one occasion, and by the intradermal on another. I cannot help feeling that, from a practical point of view, the intradermal is a far more satisfactory test than the subcutaneous. If the subcutaneous test has to be done it should be done properly, and to be done properly by my method it is a very arduous piece of work. When we test subcutaneously we take the temperature every three hours for twenty-four hours before the test and every three hours afterwards. One of the difficulties was that when taking tests before injection we had one complete reaction. As the owner of a herd, and one very anxious to maintain the herd free from any animal that reacts to the tuberculin tests, I cannot afford to take a risk. From your point of view, you come and test my herd, and do the best you can. It would take a terribly long while, and you would not agree to do it the way I want it done. But if I allow one animal in 150 to go through the mesh when it contains tuberculous lesions, I am running a risk of infecting my herd. Therefore I have got to be more careful than you gentlemen may feel is necessary. My own belief, based on what I see, is that if perfectly carried out the subcutaneous test is an extraordinarily good test. I have built up my herd with the help practically, almost solely, of the subcutaneous test. We have used the ophthalmic test, which I do not care very much about. But there are so many pitfalls apart from the question of the potency of the tuberculin—there are so many pitfalls with regard to taking temperatures. It is a tremendous job, and I am perfectly satisfied that nineteen times out of twenty when we hear of the subcutaneous test not working properly it is because the test has not been perfectly performed. (Applause.) My thorough agreement with the Medical Research finding is that this test

with the double intradermal can be so much more easily given by your good selves than can the subcutaneous, owing to the impossibility of living on a man's farm and taking temperatures as often as I, at least, would wish the veterinarian to do. An intimate friend of mine had thirty-nine animals that he believed to be free from tuberculosis, as shown by subcutaneous tests. He had been testing for some years, but after reading the report of the Medical Research Council he had the animals tested with the double intradermal test, and thirty-seven out of thirty-nine went down. I do not believe it was the error of the test, but the error of the way it had been carried out in the past. He had *post-mortem* examinations carried out on a certain number of animals and they contained the tumour. There are two other comments I would like to make. Mr. Male has referred to the results of some work that he has been able to accomplish under the Tuberculosis Order, but the Tuberculin Committee of the Medical Research Council would approve, I think, of my taking this opportunity of suggesting that any veterinary surgeons who are carrying out work under the Tuberculosis Order, who are willing to help the Medical Research Council in this respect, should write to the Secretary of the Tuberculin Committee. Their doing so would be of very great assistance. I know that some veterinary surgeons are doing the work to which Mr. Male referred, and carrying out the intradermal test before *post-mortem*. The other point I would like to make a comment about is with regard to certain criticisms that were made in the *Veterinary Journal* in regard to the Medical Research Committee's Report—it is a personal matter, and I am only concerned with it from the point of view of accuracy. Sir John M'Fadyean said in that criticism that my herd was notoriously permeated with tuberculosis. Sir John M'Fadyean, when he made the statement, forgot that not long ago the herd was tested and no animal reacted. The herd was tested by the subcutaneous test, and if any animal reacted she was sold next day as a reactor. When Mr. Sheather came down to test them, both with regard to the intradermal and subcutaneous tests, his final report was that there was no reaction.

Mr. Hayhurst, the Recording Secretary, read a note of Mr. ROBIN CATMUR'S remarks, as he was unable to be present, as follows:—

I have been deeply interested in the subject of tuberculin testing for a considerable time and just as deeply, dissatisfied with some of the methods employed, both as regards their application and the risk of error due to unavoidable external interference; even with the exercise of the greatest care the interpretation of results has been in many instances most difficult and worrying.

After some experience of the double intradermic method of tuberculin testing, I found many of our previous difficulties and possibilities of error were cleared away or very sensibly diminished. This led me to adopt the test widely and during the past three years I have applied it to some thousands of animals, with the result that I am prepared to assert that the double intradermic test is easier, both of application and interpretation, than the subcutaneous method; is far less disturbing to the animals tested and less irksome to the owners—it interferes scarcely at all with the normal routine.

I accept the accuracy of the test on the evidence of those who have been able to follow up the test with the necessary *post-mortem* examinations; but in those cases where I have been able to make *post-mortem* I have not found the test in error.

Professor Buxton's paper is most interesting and comprehensive, but he has dealt with the technique purely from an academic standpoint and I will confine any comments I have to make to a consideration of certain points which have arisen in a practical application of the test in the field on a large scale.

Technique.—I do not consider shaving of the skin to be necessary; a very small area can be clipped with scissors, and in such breeds as Channel Islanders, at certain times of the year, even this seems unnecessary. Owners of "Show" animals very much object to shaving.

Where shaving is not practised there has been observed a characteristic erection of the hairs over the œdematous swelling in case of reactions. Even where the œdema is slight this is very noticeable and appreciable from a distance.

I do not regard the all-glass syringe with separate needle, recommended by the Committee, as the most suitable for making the injections on a large scale; it is fragile, the piston is likely to slip, one dose only can be conveniently filled into it at a time and it necessitates several stages in the operation of injecting the tuberculin. I have now used for some time a dental syringe, made for me by the Dental Manufacturing Co., with barrel of thick glass, capacity 1 c.c. graduated in one-tenths; this can be filled to capacity, the dose being quite visible, in fact, magnified; the needle used is ⅛ of an inch long in a short nozzle fixed to the syringe. With such an instrument, once the skin is grasped between the first finger and thumb, the injection can be made in one stage; the resulting pea-like swelling can be felt between the thumb and finger as the injection is being made, and the operator can proceed from animal to animal with rapidity. A great pressure can be exerted with a syringe of this type on account of the special grip.

Observations and Measurements.—Whilst measurements are of great interest and are necessary for the purpose of charting results, I consider them to be quite superfluous in the interpretation of the latter; particularly is this the case where the reaction is slight. Stress has been laid on the fact that the character of the swelling is all important. In my view, too much attention may be paid to measurements, and the interested layman is already beginning to look for something concrete in the interpretation of the test, in terms of millimetres. Very soon I anticipate that auctioneers will require from veterinary surgeons definite measurements to be added to intradermal certificates, as they have in the past in the case of the subcutaneous test, either set of figures being equally likely to be fallacious.

Contrary to Professor Buxton's observations in those cases of reactions in bulls which I have seen, the reaction has always been most massive and characteristic. In many cases I have found the tail end of a fractious bull by far the safest end at which to make an injection; in such cases the sub-caudal fold has been used as a site and the double injection has left no room for doubt as to a correct interpretation. I have no hesitation in asserting the necessity

for the second injection; in many instances the primary dose has not produced a characteristic reaction in known tuberculous animals, whilst the second injection has left no doubt.

I do not consider this test the last word in tuberculin testing, neither do I consider it "fool proof." It requires care and accuracy in its application and some experience. No test of this nature can be carried out by rule of thumb methods or by the unskilled; but there is no doubt that where it has been intelligently employed by the method recommended by the Committee it has helped considerably to clear away the clouds which were beginning to engulf the whole question of tuberculin testing and the building up of tubercle-free herds.

Mr. J. C. COLEMAN (Swindon): I am glad to have an opportunity of giving my humble opinion as to the efficacy of this intradermal test in support of Professor Buxton. I would like to call attention to the remarks of some Continental scientists in which they say they conducted experiments with all three tests with the same results. I have on many occasion applied three tests, and have obtained no reaction to the subcutaneous or ophthalmic tests, but have had the reaction to the intradermal test. I rather differ from Professor Buxton as to the ease with which, according to his remarks in the paper, he would lead you to believe that the diagnosis was made. I can assure you that it is a very difficult thing, as I have in my records many instances where the swellings have gone up to as much as 16 millimetres, the original being probably 4. It is difficult to know whether it is painful, though of course it is easy to know whether it is soft and œdematous. I impress upon any beginners the importance of making quite sure, by following with a *post-mortem*, as to whether it is a reaction or not. Size is nothing; the character of the swelling is everything. I would also like to endorse what was said as to the necessity of a second or sensitising injection. You invariably get a reaction then. My reason for favouring the intradermal test is that all other tests are dependent more or less on surroundings and conditions generally, but the intradermal test cannot be interfered with. You inject and turn the animals out, repeat after 42 hours, and at 72 hours you have them all in and take measurements. My experience of the subcutaneous test is that unless it is conducted under laboratory conditions it is unreliable. I have known even turning out cows to water and taking them home twenty minutes afterwards, to cause a rise of temperature. I am quite positive that the subcutaneous and ophthalmic tests are very much dependent upon surroundings and many other things. With the ophthalmic test, when you have a reaction it is absolutely definite, but I regret to say that I have hundreds of cases where I have had reactions with the other two tests, and no sign of any with the ophthalmic. The one principal thing in favour of the intradermal test, to me, is that we know that some dealers, if they wish to get a beast passed, know that by injecting a dose of tuberculin they deter you from having a reaction within a week afterwards. That does not apply to the intradermal test. You can test three or four days following at different places and obtain a reaction.

Mr. T. PARKER (Newcastle): Two or three experiments I have had the opportunity of carrying out in the last three years compel me, from experience, absolutely to agree with everything I have heard from Professor Buxton and Major Dunkin this morning. With regard to the subcutaneous test, I am convinced that whenever veterinary surgeons have applied the subcutaneous test and got a reaction, and the animal has been slaughtered, and no evidence found of the disease, the whole question was dependent on what Professor Buxton referred to as the acid test, that is to say, how was the *post-mortem* examination made? I will give a few figures. There were 54 animals which had been tested by the subcutaneous test and had given positive results. I have had the opportunity during the past three years of making a *post-mortem* on those 54. They were tested at various periods prior to slaughter, and in no single instance have I failed to find tuberculous lesions. Within the last month I had eight cattle, ranging from nine to fifteen months old, tested by the subcutaneous test. Out of that number one gave a reaction. Within the following fortnight I applied the intradermal and ophthalmic tests, and the result was a definite reaction in exactly the same animal as had reacted to the tuberculin test a fortnight previously. That animal I slaughtered and found lesions of tuberculosis. Of 48 cattle that I have had the opportunity of subjecting to the subcutaneous test during the last three years, and which, within perhaps three or four days after the completion of the test, I have had slaughtered, and made a *post-mortem* examination upon, lesions were found in eight of the cases which had given a negative result and in one which had given a doubtful reaction; that is to say, nine errors in 48 animals, or 18 per cent. Now at first sight that may appear rather serious in this sense, that people may imagine that when they are testing cattle and do not get a reaction there may be tuberculosis present notwithstanding. There may be, or there may not be. I am speaking of cattle now that I have had to deal with apparently in a sound, healthy and robust condition—animals that we inspect four times a year and keep under observation—and the *post-mortem* examination of those few animals which had given a negative result showed that the lesions were so placed that the animals would probably have been no great danger to the herd in the meantime. But there it is, you have a number of animals that have passed the test and have lesions. Of the animals tested by intradermal and ophthalmic methods I have only done a small number, and have only had one case where I did not get a reaction and find lesions. That was an animal in perfect condition, two years old; in fact, a Christmas beast. At the *post-mortem* we found a small lesion about the size of the head of a small collar stud embedded in a lymphatic gland. I took the gland, together with the lesion itself and tested and got a positive result. I have in mind a case where an animal had passed the subcutaneous test three times in succession within eighteen months. I slaughtered the animal three days after the last test and jound a similar lesion in one of the glands, about the size of a pea. I took the lesion out, passed it through a pair of guinea-pigs and got a positive result. The small amount of experience I have had, so far as the subcutaneous test is concerned, goes to show that danger lies not with the animals that give positive results, but with animals who pass the test. I think Professor Buxton has very clearly shown to us how the intradermal test can deal with animals that have been doped. I had one case of an animal giving a positive

result for two successive years and the third giving a negative result. I *post-mortemed* within three days after the negative result and found definite lesions of tuberculosis. I was wondering if it is possible for that to be an illustration of an example in which, by successive testing and getting results by the subcutaneous method, you arrive at the stage when you get a negative result notwithstanding the lesion ? I wish to express my thanks to Professor Buxton, and to say that I am perfectly certain that everything he has said can be relied upon.

Mr. W. Steele Bodger (Dewsbury) : I have a few notes which I should like to read. Mr. Male has spoken of the way in which he adopted the intradermal test with a staff ; that made me envious, for I am myself my only staff. During the last fourteen or fifteen months I have tested some 800 head of cattle, and with the intradermal test I have never found any error when I have had the opportunity of making a *post-mortem*, but have had some difficulty in determining whether it was a positive or a negative reaction. I attribute this difficulty chiefly to the fact that I attempted to shave. I do not think shaving necessary or advisable. For one thing, safety razor blades are not given away, and after a first effort some months ago to shave a herd of 100, I came to the conclusion that I must either give up shaving or else apply for a subsidy to purchase blades. Almost invariably I got abrasions of the skin, for I could not shave them all myself, but had to leave it to the cowmen. Even with a heavy winter coat I only find it necessary to make a light clip of the scissors to demonstrate where my operation has been made. At first my casualties with needles were great, but about a year ago I had handed to me a syringe manufactured by Down Brothers. It has this advantage over the ordinary needle, that instead of the needle fitting over the nozzle of the syringe the nozzle of the needle fits inside the barrel. I have never found it shift under any pressure. Whenever possible I insert the needle and syringe at the same time. Some twelve months ago I sent to Birmingham six heifers which had previously reacted to the subcutaneous test. Professor Buxton tested at the Corporation yard, and I believe that five out of six reacted to the intradermal test. They had all been tested subcutaneously in April. In none of these cases was any sign of reaction shown on the first injection, only on the second ; but I do not think I have ever had a reaction at the second operation which did not show some signs at the first. I think the intradermal test is particularly suitable to young heifers and yearlings. These are very difficult to handle. I had a bunch of heifers tested at six months old by the subcutaneous test and all passed. They were out for some time at Christmas, and I did not test, but a month ago I tested and sixteen out of nineteen showed tremendous, extraordinary reactions to the intradermal test, although when they had passed the subcutaneous test they had been immediately taken from the farm and put on another farm where they were suckled from cows all of which had passed the test. In one or two instances after the primary injection I have noticed a swelling occur within from ten to twenty minutes. I have found difficulty also in differentiating in pain, especially after shaving. If the animal's neck is shaven it gets painful. In one or two cases sloughs have occurred when there has been a very gross reaction. In one case I found a swelling six months afterwards ; it was exactly the same, on the sme site. It had never gone away. One heifer gave me a negtive reaction to the subcutaneous test, and six months later I tested by the intradermal method and got a very doubtful reaction. She was an exceptionally fine heifer, and I was foolish enough to give her the benefit of the doubt. On the last occasion she reacted. We finished her off and the *post-mortem* showed one tuberculous lesion the size of half-a-crown. Major Dunkin mentioned, in regard to shaving, that if the animal reacted after being shaved the farmers did not like it, because they could not get out of it when the animal had reacted. I think that one argument in favour of shaving but, from the farmers' point of view, especially at present, one cannot be always scrupulous like that. I hope the day will come when people who buy female stock will always do so subject to test, but I do not see anything criminal in selling an animal that has failed to pass a test. One must sympathise to some extent with the motto of the farmers : "Do unto others as they do to you, and try and do them, for they are sure to do you." (Laughter.) One thing about the subcutaneous test is that I think we as a profession are largely responsible for having brought it into disrepute. It is the practice of some veterinary surgeons to leave the thermometers for other people to take the temperatures. That brings the test into disrepute. If one is going to use the subcutaneous test one must take every temperature oneself. (Hear, hear.) Largely, the discrepancies occur because this has been left to somebody else to do.

Mr. R. Simpson (Cumberland) : I want to confine myself to one remark—Mr. Male has referred to the question. He has said that there are certain cases of generalised advanced tuberculosis which fail to give reaction to the subcutaneous test. I have had, during the last eight or nine months, a considerable amount of experience with the intradermal test, and in two cases which were showing definite clinical signs of tuberculosis—tested not for the sake of arriving at a diagnosis by the aid of the test, but tested for experimental purposes—I failed to get any reaction whatever. I have had two distinct and separate failures from the test. Was the technique in those cases at fault ? I think not, for I had a fair amount of experience beforehand. I questioned the potency of the tuberculin, but I was using stuff prepared by Professor Buxton, who assured me the tuberculin was all right. In the other exception the animal was saturated with tuberculin toxin, but no reaction could be obtained. Has Professor Buxton any similar experience ? I am a convert to the test ; the saving of labour and expense make it a recognised test.

Major P. J. Simpson (Maidenhead) : I did not intend to speak, for I have not had much experience of the test. I have some 700 animals ranging from quite young calves, a few weeks old, up to aged bulls. My experience has converted me to the intradermal test as regards efficiency and truthful recording as against the subcutaneous test. To the ordinary practitioner working in the field it is something of a godsend, and if we as practitioners can rely on the potency of the tuberculin and the statements made by the Research Committee as being correct, then I think we have a very valuable asset with regard to help in trying to free our herds from tuberculosis. There are

one or two matters in which I disagree with one or two previous speakers. With regard to shaving, all the animals I have tested I have always shaved. I agree that I have had perhaps less than a half per cent. of a little trouble with the skin where it had been roughly shaved, but in other respects I have not experienced any trouble; and I am personally of opinion that it is a distinct asset in helping one in determining as to a reaction, as to whether it is a definite sore reaction or a hard pus-like swelling. So many people have covered the same subject that it is inadvisable to go all over the ground again. It is merely the experience of the shaving and the rough outline of the experience I have gained which has converted me to the opinion that this is a very reliable test indeed.

The Reply.

Professor Buxton: The hope I had in mind when bringing the paper forward for discussion has been amply justified. No doubt the discussion which has taken place to-day cannot have failed to clear the ground in many respects, especially in the case of some people who have not applied the test on any very extensive scale. There is little to which to reply, but one or two interesting points have arisen. I am glad that Mr. Male pointed out that I did not condemn the subcutaneous test. I have never condemned it, and nothing is further from my mind. My actual attitude towards the whole business is that stated in the beginning of my communication, that if we can do anything to clear up the little difficulties that arise we shall have done something towards simplifying the testing of tuberculous animals. Mr. Male went on to elaborate his faith in subcutaneous tests. I would be the last to reproach him for that. He told us he has built up tuberculosis-free herds by the subcutaneous test, and I have not the slightest doubt that is so; but he was bound to bring before your notice cases, where animals were supposed to be free because of the subcutaneous test, but were afterwards found not to be free, this fact being confirmed on *post-mortem*. You can never have any certainty as to the efficacy of a test until you have slaughtered the animal. Mr. Male says it is very nice to have a temperature chart: it is more definite and easier to interpret than a measurement chart. All charts are nice, whether temperature or measurement, so long as they are reliable. In referring to the extent of the character of the swelling as distinct from the measurements, Mr. Male said that he did not attach much importance to pain; and a little later he said the character of the swelling was most important. I think he had in mind the non-specific pain which might be due to too vigorous shaving. One other point, the repeated handling of the area. When several people have been examining the swelling on the neck of an animal I have noticed that when the first man touched there was no evidence of pain. Then two or three other people have handled it and the animal has increased in the degree of flinching that it shows. One observes that almost every day one applies the test. I cannot agree with taking the temperature at the sixth hour for this reason, that Mr. Male says that it is not necessary unless the animals have been recently tested before. That is the point of the Minister of Health. That is the point of anyone who suggests that it is advisable to take the temperature at the sixth hour. If one is in any doubt, it is necessary to be quite sure that in labelling a herd as tubercle-free it shall be really tubercle-free. There is no reason why the two tests should not be used alternatively, and I think probably that is a very good scheme. Mr. Buckley points out that the work done by the Medical Research Council was at the request of the Minister of Health. I am sorry that for the moment it had slipped my memory. Many speakers are of opinion that shaving irritates the skin. I quite agree that in some cases it irritates the skin, particularly when done with a blunt razor. It is not uncommon to see an abraded surface left for the injection of tuberculin. In such cases you get all sorts of things happening afterwards. But, provided the area is shaved with care, there is not much objection. I like it because I like to have a very clean area on which to make observations. On the other hand, I am quite convinced that adequate clipping is sufficient. Mr. Buckley also referred to one very unfortunate experience of a herd of animals which was later tested and found to contain a number of reactors. Some of these were slaughtered and found to have tuberculosis. I came in contact also with that unfortunate experience. There is only one other point to which I wish to refer, and it is in connection with the lesions which one finds on *post-mortem* examination. It not unfrequently happens that an animal gives a good reaction to a tuberculin test, subcutaneous or intradermal, and when slaughtered it is practically impossible to find any lesions. Unless one has adequate facilities, there is a tendency to say that the animal was not tuberculous. We have come across four such animals, and in each case when the secretion of the lymphatic glands from these animals has been injected into guinea-pigs it has produced lesions, but in other cases the lesions were hard and calcareous. Mr. Buckley also referred to the experience of Mr. Simpson. I have been in correspondence with him, but I am quite unable to ascribe any reason for what he speaks of other than that of the animals being so extensively infected that they had, for some reason or other, been unable to respond to the stimulus, just as so often happens with the subcutaneous test. I do not think that there are any other points which require answering. I must thank you for your attention.

Major-General Sir John Moore: It gives me very great pleasure indeed to propose a vote of thanks to Professor Buxton. I have had no experience myself with the intradermal test, but all the same I am very much interested in the subject, and I think his method marks a great advance in our professional methods of diagnosing disease. I would like to tell the story of a similar intradermal testing with regard to horses. I have, as perhaps you all know, had experience of testing for the detection of glanders, and as Director of Veterinary Services with the Expeditionary Force in France I used a matter of 1,000,500 intradermal tests. Perhaps a story of that testing would interest you. It has no reference to a vote of thanks, but it is a very interesting one, and I think it is perhaps one of the triumphs of our profession in war really worth recording. We had no trouble at all with glanders in France, and glanders, I may tell you, has been the very Napoleon of disease of veterinary service in war. But in this instance it was absolutely killed by a graduate of the Staff College. Our experience in France with that

method was, as I say, really not only a triumph for us, but it was the means of keeping us entirely free, and in the end of time, saving our country from any introduction of the disease. The story is this: we started with the subcutaneous method. We had a menace in the first year of the war and we determined to test the whole of our horses. We did so. We began with the subcutaneous method. But a very excellent young French officer, Duville by name, came to us and told us that we were behind the times. They had adopted the intradermal palpebral method of testing. From that time we adopted this method and discarded absolutely the subcutaneous method. It simplified our work in France. We never took any temperatures. There was no necessity. Any fool could tell a reaction to the intradermal palpebral method. Moreover, we could test our animals and let them do their work at the same time and that was very important when ammunition columns had to take their ammunition to the front and supply columns had to get supplies up. I am certain that if ever we again go to war we shall use that method of testing. It has occurred to my mind whether the intradermal palpebral method could be tested. I do not know whether there is any difficulty, unless it is an anatomical one, the eye of the cow being different from the eye of the horse. We found it very rapidly done; one officer could do 240 horses in forty minutes. Perhaps at some time Professor Buxton could enlighten me on the subject of whether the palpebral method is or is not suitable in tuberculin testing. I shall formally move a very hearty vote of thanks to Professor Buxton for his most excellent and instructive paper and to Major Dunkin, Mr. Male, Mr. Buckley and the other speakers for having spoken on the subject (Applause.)

Major SIMPSON: It is with very great pleasure that I rise to second the vote of thanks proposed by Sir John Moore. I do so because I count myself fortunate in being one of Professor Buxton's personal friends. I happen to know the extremely busy life that he leads and I think that this Section is extremely fortunate in being able to obtain him to come down and give such an interesting paper as he has given to-day. Unfortunately, the time has passed too quickly and we have reached the end of the proceedings. I must say no more, but I echo all your sentiments.

The vote of thanks was unanimously accorded.

Mr. BUCKLEY: I have asked the Chairman's permission to take up your time for ten seconds. We are all interested in the test from the point of view of the public and the use of milk. I want you to consider, when the time arrives, whether you think it is a fair thing that milk should be called Grade A milk if it has not come from a herd that contains no animal that reacts to the tuberculin test. Under the orders of the Ministry of Health, Grade A milk comes from animals physically tested and not tuberculin tested.

Mr. T. EATON JONES (Liverpool): I have a very pleasant duty to perform, to propose a vote of thanks to Mr. Alderman W. Phené Neal for the way he has presided over our meeting. I have had the pleasure of knowing him for a great number of years and each year he seems to take more and more interest in the veterinary profession. We have progressed very greatly in recent years, but we want to progress a great deal more, particularly with regard to meat inspection. I feel that we should try and get Alderman Phené Neal, taking the interest he does in our profession and holding the position he does in public life, into Parliament. He has the power of oratory, and I know he has the money. If we could persuade him to go up for some constituency I feel sure that every one of us who is in the position to do so would do our utmost to get him into Parliament. We are a very small profession, probably the smallest in the Kingdom, and without somebody to help us in Parliament we can do very little either individually or collectively. I feel sure that at some future date, I hope at the next election, we must try and do what we can to get Alderman Phené Neal into Parliament. (Applause.)

Mr. TROTTER (Glasgow): I second the resolution. Possibly I am the oldest of the veterinary inspectors of meat, and I know the uphill work the veterinary inspectors have to face. If we had men on our public bodies like Alderman Neal our difficulties would be to a large extent overcome, and our position would be much better than it is to-day.

The vote of thanks was carried by a hearty round of applause.

The CHAIRMAN: I thank you very much for the very hearty vote of thanks which you have been good enough to give me. I have been interested in all this work for a good many years, and I have found that as each year passes we seem to be making definite progress. I think there is no doubt to-day that we have made definite progress in the very interesting subject of tuberculosis. There is one question which I should like to ask Professor Buxton to answer, and that is what is the definition of a tuberculosis-free herd, because I believe I have read that we have some 400 such herds in the country. I have also read a good deal about Lord Astor's herd, and the question arises in my mind whether in the case of a herd that is constantly being tested, and from which when reactors are found they are withdrawn and replaced by other cows, it is absolutely fair to say: "Here you have a tuberculosis-free herd," when, as a matter of fact, it only becomes so because of constant attention to the question of withdrawal of animals. We want to get to a point where we shall have really tuberculosis-free herds and breed on. Then the question arises whether we shall ever succeed in that if we insist on the in-breeding which has taken place in this country ever since 1892. That matter is being investigated in another place in which I am equally interested. Another point is that we here have devoted our attention for parts of two days to matters which, to me, appear to be vital in the interests of the country. But having done that for parts of two days we put up the shutters and only take them down twelve months hence. It seems to me that amongst the veterinary surgeons of the country there might be sufficient enterprise perhaps to have a Session of say two days some time in the winter, and I think it would probably be in the interests of all of us if a suggestion were made to the Council of the Royal Sanitary Institute so that we might have a Winter Session, first at one provincial town and then at another. If we could get together an audience such as we have here, and pursue our work, and so do away with this long interval, which I think is highly undesirable, we should make still greater progress than we are making now. I am rather sorry that Mr. De Vine was not here to-day. I remember a very interesting paper which he read, I think at Folkestone, in which he told us that out of 680

cows he found that the reaction was 38.2 per cent., and I think he said the doubtfuls were 10 per cent. If he had applied his modern test the question to my mind is whether that ten per cent. would not have gone over to the 38.2 per cent., in which case we should have got nearly 50 per cent. of the cows he tested, which were part of those supplying the town of Birmingham. If I follow on certain other investigations I believe I have not unfairly stated the case when I say that if the whole of our cows from John O'-Groats to Lands End were tested we should be nearly on the border line of 50 per cent. That being so, the veterinary profession have a very great deal of work and responsibility on their shoulders, and we ought to keep these meetings as alive as possible. We should put our shoulders to the wheel and see what we can do to reduce what is, to my mind, a terrible scourge throughout the length and breadth of the land. I thank you very much for the vote of thanks. I am always interested in your work and am at your disposal. As regards the suggestion that I should go into another place, my affections are in the City of London. I have been a member of the Corporation of London for over thirty-three years. I had an opportunity of going into Parliament when I was 27 years of age, but I chose the old City of London and I have never regretted it. I believe I can serve you there as well as in another House, where they do not always listen with the care and attention some of you gentlemen give. (Laughter and applause.) Before we separate, I should like an expression of opinion as to whether you think any good would be done by making a suggestion of having a meeting somewhere.

Mr. J. S. LLOYD (Sheffield): I remember being at a meeting at Birmingham where we dealt with the question from a psychological point of view. I wonder if the suggestions thrown out by the Chairman could be taken to the Council of the Institute so that we could have something on similar lines for our bi-annual meeting. Mr. Neal mentioned the veterinary section, but ordinary medical men and also sanitary inspectors, are interested in public health, and I think it would be wiser if the meeting were a little wider than if simply occupied by veterinary surgeons. I support Alderman Phené Neal's proposal to that extent.

Professor WOOLDRIDGE (London): With regard to the suggestion from our President, I would like to point out to him that similar meetings are continuously being held throughout the year by veterinary inspectors, that these problems with regard to meat inspection, milk, and so on, are being discussed under other auspices. But that need not throw a damper on the suggestion. As pointed out by Mr. Lloyd, other bodies might be brought into more intimate contact with the discussion than is the case at present. Medical Officers of Health are brought into contact with us at the Royal Society of Medicine. But I have some doubt whether the second meeting would be attended so successfully as is the annual meeting, and the last thing I would desire would be for any such meeting after being organised, not to be a really successful one.

The CHAIRMAN: We will leave it for the time being.

Mr. GOOCH: Would it not be a good thing that the suggestion thrown out by Mr. Buckley as regards Grade A milk should go as a recommendation from this Congress to the Sanitary Institute? If so, I will propose it.

The CHAIRMAN: I cannot put the resolution unless it is written out.

Mr. GOOCH: I bow to the feeling of the meeting.

The CHAIRMAN: A matter of such importance ought to be the subject of a prior notice, so that everybody who is connected with this Section should have an opportunity of reading that notice, and if they think it of sufficient importance could come and discuss it. It was one of the things that I had in my mind, as a matter of fact, that would make it desirable that we should meet in conference a little more often than we do. It is a matter you might very well bring up. Probably, if you gave a notice of motion to the Council of the Sanitary Institute, and asked them to convene a conference at 90 Buckingham Palace Road on the subject, you would find you could carry out your wishes, in which case we should have a special meeting on this particular matter. I think now all I can do is to wish you good health and good luck, and may we meet again this day twelve month.

This terminated the proceedings.

Through Irish Eyes.

Among the "Notes by Noa" contributed to the September number of *The Irish Breeder*, appear the following:

"We give some extracts on the extra cost of producing Grade A Tuberculin Tested Milk. The idea has only recently occurred to our people, although for twenty years and more the Veterinary Officer (Colonel Brittlebank) of the City of Manchester, has been getting clean milk for the city. Then the County of Lanark, which supplies most of the milk to Glasgow, has, through its Veterinary Officer and my old college friend, Mr. Hugh Begg, been able to get the dairymen to do things right. Later, Dr. Stenhouse Williams, of Reading, became a pioneer milkman for the South of England. But, coming nearer home, the Veterinary Officer of the City of Dublin, Mr. Dolan, has done wonders:—At a recent conference the Medical Officer of Health said 'That at present there was no dirty milk on sale in Dublin; the Veterinary supervision had got rid of it.'

"Belfast, I am afraid, is behind a bit in this social work. It appears that the people will just give as much money for dirty milk as for clean milk. The doctors, however, are about to instruct these careless or ignorant citizens. The supervision of this new and on-coming idea will shortly be attracting attention, when the Ministry of Agriculture will require to make up its mind whether it is an agricultural or a veterinary job. If one takes it that the medical officer is the man to say whether a human house is sanitary and suitable, whether the food is wholesome, and whether the man is free from disease, then by every process of reasoning, the veterinary surgeon is the only man capable of advising on byres, the food for cows, the milking of cows, sterilisation of utensils, making bacterial counts, also the soundness and general fitness of the cow herself to produce wholesome milk. We have many highly trained young veterinary surgeons capable of doing all these things. I trust it will not again be a case of going abroad and fetching over an Agricultural College product to do the work, while the North of Ireland Veterinary Medical Association continues to slumber.

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"When out at Stormont last month, I called to see Capt. J. P. Rice's show—that is 'The Animals' Disease Research Division' of the Ministry of Agriculture. The Captain has two very efficient assistant veterinary officers, viz., Mr. Wilson and Mr. Lamont. The laboratory is beautifully arranged on one side of the rectangular farmyard, and in a line with the 'Seed-testing Division.' The buildings on the other three sides are occupied with experimental animals, and with cows in for sterility treatment. The inside fittings of the laboratory (which is divided into four rooms) do great credit to Capt. Rice's architectural

ability. It is evident he knew exactly what he wanted and got it done. The floors are tessellated, smooth as glass, and with rounded corners and edges next the walls, so as to leave no place for the growth of bacteria. The walls have a polished impervious plaster, and the light is ample. When you enter the laboratory you see a very large room surrounded with test tubes, dyes and *post-mortem* specimens. A door to the right takes you into the private room of the Director (Capt. Rice). One door to the left takes you to the *Post-mortem* room, and another door, also opening out of the main compartment, takes you into the microscopic room, where the agricultural students examine specimens and get instruction on diseased tissues. Out of this you go into the incubator room where the microbes are grown artificially, and where sterilisation takes place both by dry and damp heat. Vaccines are also grown in this room, and they can be attenuated or made sterile to suit the particular therapeutic condition required. It is indeed the neatest and cleanest little laboratory I have inspected on this side, human or animal. It puts one more in mind of what one sees on the Continent. Live and dead pigs, poultry, sheep and cattle are constantly being sent in by country veterinary surgeons, they being only too glad to get a correct diagnosis made of certain of their puzzling cases. In time new buildings are sure to be required, as well as a great many additional veterinary officers, if the work is to develop to its logical conclusion—that is, a great economic agricultural movement known to us as Prevention. Once it has been determined that such diseases as pneumonia in young pigs are due to a certain specific organism, that organism will require to be grown, and the prophylactic and curative vaccines made. Such work requires a staff in itself. I have seen some such work done in the monster laboratories of the Department of Agriculture, U.S.A., where hundreds of Veterinary Surgeons are employed in the prevention of disease among the farm stock of that country. I had also charge of the British Remount Laboratory in America, where we used the blood serum from 100 hyper-immunised horses to prevent pneumonia in our horses and mules before shipment. And I must say that I have nowhere seen a more promising nucleus than we of Northern Ireland have established at Stormont. There is no doubt that it will yet be a great blessing to the stock owners of Ulster.

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"I went up to Dublin last month to attend the Annual Conference of the National Veterinary Association. Representative veterinary surgeons were there from the British Isles, and also from abroad. I never saw Dublin looking so busy or so clean. The Commissioners are governing the City well. If our Councillors and Aldermen had seen it before they voted to keep themselves in office they would probably have voted themselves 'a seat in Coventry.' Who knows! In any case I have been converted to Commissioner Government. Many things were discussed which were of interest to farmers, amongst them sterility and abortion in cattle, disease of poultry and pigs, and the importance of clean milking of dairy cows. A Dane, called Professor Nielsen, dealt very ably with sterility, and afterwards gave actual demonstrations on four cows as to how he proceeded to deal with the trouble. Our own Capt. Rice dealt with the diseases of poultry, and a Dutch professor opened the discussion. Professor Craig read a paper on Swine Erysipelas, a disease of economic importance. Strange to say, only five Ulster veterinary surgeons were present out of a total of 120. Those that did go are useful men, who want to be more useful to their clients. I may mention—Mr. Kelly, Portadown; Mr. Hart, Enniskillen; Mr. Johnston, Jun., Belfast and Capt. Rice. It would be a great thing if the 'National' could be brought to Belfast. If Capt. Rice, gets going, and does something of international value, it may be coaxed to come in five years' time.

"The Dublin and South of Ireland veterinary surgeons rose well to the occasion. Mr. Louis Magee was a host in himself. He, all by himself, chloroformed, roped and castrated a colt; and afterwards operated on a rig through

(*Continued at foot of next column.*)

CLINICAL AND CASE REPORTS.

The Administration of Capsules to Sheep.

By R. F. MONTGOMERIE, B.Sc., M.R.C.V.S.,
(Veterinary Adviser, University College of North Wales, Bangor.)

During one of the many "unofficial" discussions which aided in making our recent Annual Congress such a pleasant affair, the administration of capsules to sheep came under review. This note is the result of a suggestion that the writer might, with advantage, record the method which he uses in the administration of capsules containing 1 c.c. of carbon tetrachloride in the treatment of liver rot of sheep.

There are certainly many modes of giving capsules to sheep. Experienced men frequently give them in a manner very similar to that used in dog practice. The characteristically-sharp lingual borders of the premolars and molars of sheep readily cause abrasions of the fingers, particularly when the subject is of the smaller breeds. In addition, this is a tedious and often uncertain procedure.

Others methods which involve the use of instruments have been recommended, but the writer has found that now described the most convenient to him. The "balling gun" used is a simple home-made instrument of negligible cost. A piece of rubber tubing about 11 inches long with a bore of half-an-inch and a wall thickness of one-sixth is the only necessary purchase. A 13-inch length of one inch square wood is cut down so that about 11 inches will enter the tubing to act as a round stilette. The remaining two inches are smoothed to act as a handle too wide to enter the tubing. The stilette should work just easily within the tube and be cut so that, when it is fully home, it is about a quarter of an inch shorter than it. This affords protection when in use. The stilette may be rubbed over with linseed oil as a lubricant.

In use, the stilette is drawn back about an inch and the capsule, dipped in oil, inserted into the chamber thus formed. That end of the gun is passed into the sheep's mouth, which is held open by an assistant, and the stilette pushed forward to liberate the capsule beyond the dorsum of the tongue

The manner in which the sheep's mouth is held open is of importance. The writer, as a general practice, places the sheep on its haunches. The

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(*Continued from previous column.*)

the flank. Mr. O'Brien, a veterinary surgeon from Mayo, operated on a yearling calf for 'worms on the brain'—same disease as gid in sheep with the same symptoms. Fortunately we are not troubled to any great extent with the disease in Ulster. Professor O'Connor chloroformed a hunter, and before the animal got up he operated on both hind legs for stringhalt, and then on the throat for roaring. Professor Nielsen, as I have said, operated on cows for sterility with much acceptance.

"Why should the profession in Ulster be content always to take a back seat?"

assistant places the forefinger (or first two fingers) of his left hand just behind the dental pad, the thumb resting on the nasal bone, and the thumb of the right immediately behind the incisors with the fingers of that hand encircling the lower jaw. Gentle traction then opens the jaws, leaving the maximum of space available for the manipulation of the gun. It is to be noted that the tongue is not held in any way. If it be included in the grip of the lower jaw, the sheep, fighting against this added restraint, raises the dorsum towards the palate, and so interferes with the free passage of the gun.

That this mode of giving capsules is quick and easy is illustrated by the fact that, with efficient catching assistance, the writer has dosed large numbers of sheep at the rate of about 150 per hour without undue haste and in no attempt to create or break records.

The detail of the gun described above may readily be altered to allow the administration of larger capsules than those for which this particular one was designed. Thus, the use of tubing with a bore of about five-eights of an inch makes a convenient gun for the administration of No. 12 hard gelatine capsules.

Ovariotomy in a Kitten.

By H. Burrell, M.R.C.V.S., Croydon.

These notes may possibly be of interest to some practitioner who, like myself, is obliged to operate single-handed, and to act as anæsthetist and surgeon at the same time.

The patient was a very active and well-developed half-Persian kitten, between 10—11 weeks old. As much fur as possible was removed, with curved blunt-ended scissors, from the seat of operation, on the day before the operation, and the part swabbed with 2 per cent. lysol.

All instruments and suture materials were boiled and left all night without the lid of the steriliser being lifted—just before the operation they were boiled again, and the tray of the steriliser was lifted out into the inverted lid of the same, exposing the instruments and silk ready for use.

The next morning the animal was anæsthetised in a chloroform box, transferred to an operating table and hobbled as soon as she became unconscious—a Hobday Inhaler (No. 2) was now brought into use.

After lathering and shaving an area in the middle line of the abdomen, just in front of the pelvis, and disinfecting with 2 per cent. of lysol, a small incision was made in the skin with a scalpel and then enlarged with the help of a director—the same process being employed with the abdominal muscles and the peritoneum.

My hands were scrubbed with soap and warm water, and antiseptic ethereal soap was used before commencing this part of the operation; but before introducing a finger into the abdominal cavity the patient was put under a deep anæsthesia, and (1) the hands were washed with ethereal soap; (2) rinsed in clean water; (3) rinsed in chinosol solution (½ grain to the ounce); (4) washed with boiled cotton wool swabs.

Every time it was necessary to administer more chloroform this process had to be repeated before the operation could be continued, and this occurred about three or four times.

The left arm of the uterus having been found with a finger, it was traced to the ovary and a Staffordshire knot was put over the latter, which was removed with scissors and the horn allowed to return into the abdominal cavity—the other ovary being removed in a similar manner.

The suture silk used to ligature the ovaries had been kept in lysol solution, but Staffordshire knots were made and nipped with artery forceps to prevent them coming undone, before boiling, ready to loop over the ovaries, without further handling, except to tie the final knots.

The peritoneum and the muscular layers were drawn together with three sutures, but seven sutures were used to close the incision in the skin—silk being chosen in both cases, though most surgeons appear to use catgut for the muscular layer.

The wound was then disinfected and dried with cotton wool and finally painted with collodion and tannoform.

Two hours after the operation the patient ate some minced fish and drank some milk and from that time onward was continually trying to find a way out of the pen, but purred readily on being stroked.

The next morning she ate well and again at mid-day, but in the afternoon her efforts to get out of the pen became so violent that I persuaded the owner to come over and I then motored them both home (about five miles).

On arrival, the kitten having thoroughly explored the room and recognising the place, finally settled down and went to sleep for a while.

I called on the third day after the operation and repainted the wound with tannoform collodion and found the animal still very active and feeding well.

On the sixth day, my next visit, I removed five stitches and disinfected the wound, which the animal had been licking, with chinosol and hydrogen peroxide solution, with which I instructed the owner to swab the wound night and morning.

Seven days afterwards I removed the last two stitches, having found the animal climbing all over the furniture as usual and feeding well. The wound had healed beautifully and the fur had commenced to grow round it.

Apparently it is unnecessary to keep an animal quiet after such an operation (as my patient took exercise freely from the time that she recovered from the anæsthetic onwards) provided that the skin sutures are numerous and placed closely together.

An Interesting Parturition Case.

By F. Beckett, M.R.C.V.S., Blandford.

On the 22nd July last, I received an urgent message to attend a heifer that could not calve.

On arrival, I found a small, first-calf heifer, which had been in labour for some considerable time.

Examination showed the embarrassing feature of two fore-legs, two hind legs and the head in the vagina.

A transverse (sterno-abdominal) presentation—right cephalo-iliac position, was diagnosed.

An attempt was made to turn the fœtus and deliver, the hind legs first; this failed.

After fifteen minutes' hard work no progress was made—the fœtus appeared to be jammed in the pelvis.

The head and fore limbs were removed. I then elevated the heifer's hind quarters by the use of pulleys and turned the fœtus, intending to deliver it in the longitudinal posterior position.

During the process of lubricating the heifer a large mass was felt and this was found to be another pair of hind quarters, belonging to the same fœtus.

This mass was well pushed back and occupied the position which would have been held by the head and fore limbs in breech presentation.

Two men with a bar applied traction and delivery was accomplished without much trouble.

The fœtus took the form of the letter Y—dividing behind the ribs into two pelvic regions, four legs and two tails.

The heifer made an uneventful recovery.

ABSTRACTS.

The Protection of Cattle against Johnë's Disease.

This is a preliminary note regarding a possible method of protecting cattle against Johnë's disease. The theory that forms the basis of the method is that animals suffering from chronic infections cannot be super-infected.

The available evidence goes to show that subcutaneous inoculation of animals with cultures of the bacillus of Johnë's disease fails to cause infection.

With the object of producing a persistent local infection with the bacillus, the authors have adopted the plan devised by Calmette in connection with the tubercle bacillus of injecting certain weighed quantities of culture with sterile paraffin, and the result of such injection is to produce, after a lapse of some forty-eight hours, a hot, painful swelling about 5-7 cms. in diameter. There is no febrile reaction. In the course of a few days the acute inflammatory symptoms subside and there is left a hard painless swelling about half this size. These bosselated fibrous lesions are very persistent, and they have been observed to persist for as much as two years without change. The duration of their persistence is not yet known. In no case has there been observed any softening or breaking down of the tissues with expulsion of the contents of the nodules. Nor has there been found in any such inoculated animal, when submitted to *post-mortem* examination, any evidence of extension of the infection.

The local lesions, when examined *post-mortem* are found to comprise a central cavity containing an oily substance, which is the original excipient, around which are grouped clusters of small nodules about the size of peas. These are embedded in fibrous tissue and have all undergone caseation. The whole lesion is surrounded by fibrous tissue which is adherent to the skin and the underlying muscles and which isolates it from the rest of the tissues.

Guinea-pigs and cattle are quite unaffected by inoculation with the caseous material.

Observation on animals at the laboratory indicated that the inoculation was quite harmless.

As it is impossible to produce Johnë's disease as it occurs naturally by experimental means, the value of the method has to be tested in the field.

Up to the present the authors have had 277 apparently healthy animals placed at their disposal for field tests. About one-third of these have been kept uninoculated as controls.

Full details of these tests are promised in a later communication. The authors content themselves for the time being with stating that they have grounds for considering the method of protective inoculation promising. On one farm where three cases have occurred among the controls none of the inoculated animals has developed the disease. On another farm where controls have become infected one animal among those inoculated has been slaughtered on account of infection. This animal, however, showed no inoculation nodule at the time of death. The authors state that this may have burst or have been absorbed, or it is possible that owing to an error the animal actually escaped the inoculation.

With regard to the suppuration of inoculation lesions, the authors note that there is a tendency for this to occur when animals already infected are inoculated. Inoculated animals react to a test material prepared from Johnë's bacillus (using *B. phlei* and not tubercle bacilli for the preparation of the media) but not to tuberculin.

[**Etudes sur l'enterite paratuberculeuse des bovides.** (Johnë's Disease.) H. VALLÉE and P. RINJARD. *Rev. Gen. Méd. Vét.* 1926. January 15th. Vol. 35. No. 409. pp. 1-9. *Ex. Trop. Vet. Bull.* Vol. 14. No. 2.].

(A Study of Vitamin Mineral Mixtures and Vitamin Products in Feeding Farm Animals. *Journal American Veterinary Medical Association.* Vol. 22. No. 5.)

The attention of veterinarians is drawn to the flourishing sale of proprietary preparations, which are represented as needful for the complete nourishment of farm animals. Mineral mixtures and sources of vitamins are chief among the above preparations, and the author herein discusses their real utility.

There are a dozen or more mineral elements essential to the animal body, but because no diet which is otherwise satisfactory is entirely lacking in any of these substances, most of the elements can be dismissed on the score of malnutrition. There are five elements, however, which do require consideration in this respect, and these are sodium, chlorine, calcium, phosphorus and iodine. Common ingredients of proprietary mineral mixtures are magnesium, sodium and ferrous sulphates as well as potassium chloride and sulphur, although there is no evidence that they have the slightest nutritional value when added to ordinary farm rations. With regard to the medicinal value of these substances, the writer tritely says that a healthy animal does not need medicine, and when ill requires specific treatment. There is also evidence that the continued feeding of small amounts of laxatives is not beneficial, but harmful. Another objection to proprietary mineral mixtures is that they are not sold with a guaranteed analysis, and in many cases the price bears no relation to the cost of bone meal, which is the best source of calcium and phosphorus. That the feeding of minerals will prevent or cure abortion is sometimes claimed, and while this is a foolish statement, there is evidence that reproduction troubles are less frequent under conditions of adequate calcium and phosphorus nutrition.

The recently-discovered knowledge regarding vitamins in medical science has led to the false assumption that these vitamins are equally important in the nutrition of farm animals. Animal foods are much richer in vitamins than human foods. Our present knowledge suggests that there is no danger of lack of vitamins A, B, and C in the food of cows, sheep and horses, and it is a waste of money to supply them. The case is different with vitamin D, which aids mineral assimilation. The best source of this vitamin is cod liver oil and it has proved beneficial when fed to poultry and swine. More important is it for the farmer to attend to the other principles of complete rationing, and to feed plenty of protein and digestible nutrients, than to pay exorbitant prices for worthless materials.

W. L. S.

There has passed away one of the links with the pioneers of our profession in Percival Percivall, Esq., of Ipswich, at the advanced age of 82 years.

Percival Percivall was the last surviving son of Wm. Percivall, whose name will always be associated with the anatomy and surgery of the horse.

DIVISIONAL REPORT.

Western Counties Division.

DISCUSSION ON DR. J. S. WHITE'S PAPER, "VETERINARY BIOLOGICAL PRODUCTS."

(*Continued from page* 822.)

The PRESIDENT expressed the best thanks of the Division to Dr. White for his very interesting and instructive paper. Concerning blackleg, he remembered an extraordinary case affecting lambs. The farmer docked about sixty lambs, and within a few days a number of them died. Then it was found he had used a knife with which he had skinned a dead yearling. In regard to tetanus, he was one of those who believed in the use of serum. He had tried parathyroid for chorea but had not had much success with that.

Mr. R. B. NELDER thanked Dr. White for coming and giving them such an able lecture, also for showing his very interesting lantern slides, which had brought out the points very well indeed, and so greatly helped in making the lecture a success. "One of the parts which interested me very much" (continued Mr. Nelder) "was the preparation of serum. Last year, when I was at Rotterdam I went over a serum institute there, and although smaller than the one mentioned by Dr. White a good deal of serum is produced there and the method was very similar to that described by him. Horses represent the largest number of animals used, and they are kept under very good conditions. They were horses of all colours, but mostly of light type.

"Anti-tetanic serum is the one most largely used in veterinary work and, provided one gets cases early and gives a big dose at the start, success is very marked. Anthrax serum is not used here in England. Cases are nearly all dead before one sees them. A fortnight ago, however, I heard of a case close to my home where the veterinary surgeon was informed before the animal died, and it lived a couple of hours. With regard to the use of mallein, horses imported into Holland are tested with it for glanders on arrival, by a rather simple method. A camel hair pencil is dipped into the mallein and, pulling down the lower lid of the eye, it is run across the eye. I have used the same method with tuberculin and have found it very simple."

Mr. MASSON: I should like to ask Dr. White why it is not possible to produce an anti-distemper serum from dogs which have recovered from distemper? I entirely agree with him with regard to anti-tetanus serum. I myself can swear by it. Dr. White says there was great fatality with white scour in calves, but we are not troubled with it in our district, though many farmers get a lot of trouble with bloody scour. Dr. White has mentioned the growth of animals being influenced by ductless glands. Doing a little experimenting with thyroid some little while ago, I picked out the most miserable looking pullet I

could find and treated her with it, and she won first prize at Newton Show this year. We are now experimenting with pituitrin, but I cannot give any results yet. As an old and out-of-date vet. (laughter) I should like to hear something about anaphylaxis. At one time I never treated a case of distemper without serum, and I have tried a few cases with the present bacterin.

With regard to the ophthalmic reaction, is it not a fact that this reaction passes off very early ?

Mr. EDWARDS : Arising out of Mr. Nelder's remarks, there is one point I should like to raise. In applying the tuberculin test by the use of a camel hair pencil what steps are taken for the sterilisation of the pencil, and is it dipped into the same bottle over and over again ?

Mr. NELDER : A fresh one is used every time and sterilised before use.

Mr. ROBERTS : I do not think I have heard anything about contagious abortion. I have been using vaccine in many herds. In about half a dozen herds it has been successful, apparently with aged cows already in calf. The big majority of them have carried their calves until full time, but with regard to heifers on infected grounds, they have aborted within a few weeks. Vaccine confers active immunity, but I should like to know how long active immunity lasts.

Further questions respecting the vaccination of dogs against distemper were asked by Mr. Masson and Mr. Wright.

Replying to the points raised by members, Dr. WHITE said he was not surprised to hear from the President that he had failed to get results with parathyroid in chorea. Personally, he had been very disappointed in cases which had come to his knowledge, and he was not surprised to know that the same thing had been their experience. He thanked the Secretary for his remarks, and was interested to hear that the methods carried out at Rotterdam were similar to those in other parts of the world. " The use of a camel hair pencil, as mentioned by Mr. Nelder, is by no means new," continued Dr. White, " I remember writing a little note on the use of tuberculin at least eight years ago, and I referred to the possibility of using the pencil for carrying out the test. It was the result of the animal becoming unmanageable that led to the use of ophthalmic discs which dissolve in a few minutes. The pencil is a convenient method, and with the disc you might lose it. I think Mr. Masson asked if it were possible to produce serum from a dog recovered from distemper. I think it is possible in a small way experimentally, but as a commercial proposition, it is hopeless ; you could not possibly do it. Attempts have been made to obtain serum from children who have recovered from measles. They have collected quite a little, but that is less than a flea bite to deal with all the measles in the world. The dog is not a sufficiently large animal to enable us to get a sufficient quantity of serum.

" You also said that white scour, as such, did not worry you, but that the condition known as black scour did. I have had no experience at all. I was immensely interested to hear of your results with thyroid. That very distinguished doctor in Edinburgh, Dr. Crewe, says he can rejuvenate fowls with thyroid, but you have taken that one emaciated bird to start with, and it has carried all before it. The only suggestion I have to make is that next year you feed the bird on anterior pituitary and thyroid and send it to the Royal Show. (Laughter.) The result of combining these two things is indeed remarkable."

Describing the different effects of injection of serum and bacterins, Dr. White said : " When a bacterin is given it makes a definite demand upon the patient, which is not the same with serum. In giving serum no demand is made upon the patient. That was why we generally use serum in an acute condition, and bacterins in a sub-acute or less serious condition. There is no reason why one should give serum in three doses. It is not a case of getting the patient more immune, but of administering antibodies. With regard to vaccinating against distemper, I believe failure is due to conditions under which the dog is kept. If you put the dog into an infected kennel you make that dog run greater risk than if he did not have his bacterin. Secondly, you must regulate your doses to the size of the dog, and start with the smallest dose. I prefer to give a dose every three or four days. In the human patient I wait a week. Again, I should not inoculate an animal when it is not fit. With regard to injecting vaccine, that is not a difficult proposition. Personally, I do not boil the syringe, because there is a danger of breaking it. I wash my syringe with ether and have given thousands of injections and have had no trouble. Incidentally, I use a little ether for painting the skin. With regard to contagious abortion, unless your vaccine contains live organisms it is not of much service. I am interested in a bovine bacterin, one of which contains *Bacillus abortus*. I am afraid I cannot say how long immunity lasts."

Mr. MASSON moved a cordial vote of thanks to Dr. White for his most valuable lecture and interesting replies to the questions asked him. They had all greatly appreciated his visit. This was seconded by Mr. BOVETT, and carried with applause.

CLINICAL CASES.

Under this heading on the Agenda, Mr. MASSON described the case of a spaniel dog stung by an adder. The usual treatment was adopted, and the dog seemed to get over it, but three weeks later the animal was suddenly taken with what the owner called cramp and spasms. The dog was brought to him and he could find nothing wrong, excepting a very strong heart beat, but there was no valvular disease. He had given it further treatment but the dog had not completely recovered. The dog would be apparently bright and well and then, perhaps at a most inopportune moment, would be suddenly taken and scream as though in pain. Last week he put it on parathyroid and calcium. Talking to a man who had had experience with tropical diseases, he was told that it

was quite possible that these attacks were due to the effect of the snake bite.

Dr. WHITE replied that what he would have done in a human being was to adminster an anti-venom serum. There was a serum in use derived from snake bite.

Mr. EDWARDS, proposing a vote of thanks to the Chairman, said the election of Mr. Saunders at Exeter was absolutely unanimous, and his conduct in the Chair that day had shown that their confidence in him was not misplaced. The vote was heartily accorded, and Mr. SAUNDERS briefly responded.

Members and visitors were afterwards entertained to tea by the President.

NOTES AND NEWS.

The Editor will be glad to receive items of professional interest for inclusion in these columns.

It is a matter of some gratification to us that periodicals of repute are making an increasing practice of reproducing material from the columns of the *Record*, of course with due acknowledgment. We notice that *Public Opinion* of September 3rd, despite the great demands upon its space as a medium for epitomizing all the vital news of the week, finds room to publish a considerable portion of our ex-President's (Professor Buxton's) address from the Chair at the Dublin Congress, while the current issue of the U.S. Army *Veterinary Bulletin* contains our complete report of Professor James MacQueen's paper to the Mid-West and South Wales Division, "Some Observations on the Examination of Horses as to Soundness."

R.C.V.S. FELLOWSHIP EXAMINATION.

An Examination for the Fellowship Diploma was held at 10 Red Lion Square, W.C.1, on Friday, September 3rd, 1926, when the following candidate was successful :—

Mr. ANTHONY SOUTH CANHAM.

Thesis : "Blood of South African Cattle."

Examiners : Dr. G. A. Buckmaster, M.A., M.D., and Dr. W. H. Andrews, D.Sc., M.R.C.V.S.

Chairman : G. H. Livesey, Esq., M.R.C.V.S.

R.C.V.S. OBITUARY.

LEACH, EDWARD HUGH, The Chestnuts, Newmarket. Graduated, N. Edinburgh, 16th April, 1879. Died, 2nd September, 1926. Aged 73 years.

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In reference to the death of Mr. Leach, the *East Anglian Daily Times* says :

"Universally recognised as one of the leading members of his profession, the deceased had had a very large and important practice in Newmarket for over forty years. He was professionally associated with most of the principal training establishments at the headquarters of the Turf. He was also one of the leading breeders of thoroughbred stock in the United Kingdom, and had important stud farms at Newmarket.

"Though crippled as the result of a serious accident several years ago, when he sustained a dislocation of the thigh, Mr. Leach led a busy and active life practically up to the last.

"He was a brother of Mr. Felix Leach, of Graham Place, Newmarket, the trainer ; and uncle of Felix Leach, jun., the trainer, and of J. and H. Leach, the jockeys."

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THE LATE MR. HENRY JARVIS.

"The death of Mr. Henry Jarvis, M.R.C.V.S., the former East Sussex Veterinary Inspector, occurred at West St. Leonards, on Tuesday, after a long illness," says the *Sussex Express*, of August 27th.

"Mr. Jarvis, who was 74 years of age, was a well-known local figure and was loved and respected by all with whom he came in contact. His practice extended over a period of 53 years and during that time he did a great deal to alleviate the sufferings of some thousands of animals. On one occasion he successfully operated on a goat that had swallowed a cricket ball.

"The son of a farmer, Mr. Jarvis was born at Burwash in 1851, and here he spent his early life. His passionate love of animals made him determined to become a veterinary surgeon, and in 1872 he took his degree at Camden Town. He set up a practice in his native village, residing at Alba House. From there he went to Battle, living at Senlac House, but afterwards went to St. Leonards, where he practised for 30 years at 85 London Road. He was appointed Veterinary Inspector to the Board of Agriculture for East Sussex and was in 1897 made the Borough Veterinary Inspector for Hastings. He was married in 1874 to Florence Clifford, of Church House, Burwash, and they had six children, three sons and three daughters, all of whom survive their father. At the age of 70 he retired from his inspectorships and went to live at Osborne House, Wadhurst, where he established another practice. He was taken ill last January and subsequently went with his wife to live with their second daughter at West. St. Leonards, where he passed away early on Tuesday morning."

WORLD'S POULTRY CONGRESS.

Although it is likely that the English-speaking races will predominate at the World's Poultry Gongress at Ottawa in July, 1927, many other nations have accepted the invitation to participate. The Spanish Government has appointed a National Congress Committee, at the head of which is Professor Salvador Castello, director of the Royal Poultry School of Spain. The Government of Finland will be represented by the Finnish Consul-General in Canada, and the Government of Poland has entrusted the organisation of the Polish section to the Central Polish Committee on Poultry Rearing, which will be represented, among contributors of papers, by Dr. Stefan Kopec, of the Polish Government Institute for Agricultural Research, and Professor Maurice Trybulski, of the College of Agriculture, Warsaw. The Scottish Board of Agriculture has appointed three delegates to attend the congress.

CORRESPONDENCE.

Letters to the Editor should reach the Office not later than by the first post on Tuesday morning for insertion in following Saturday's issue.

All correspondence must bear the name and address of the contributor for publication.

The Editor does not hold himself responsible for the opinions of his correspondents.

Inspector's Certificate Conclusive Evidence under the Diseases of Animals Act of 1894.

To the Editor of The Veterinary Record.

Sir,—The case of Jackson *v.* the County of Durham Quarter Sessions, which recently came before the King's Bench Division, and where it was held that a veterinary inspector's certificate was conclusive evidence as to the existence or non-existence of disease, and bound to be accepted in a court of law, seems to be causing a considerable amount of uneasiness amongst the farming community generally, and no doubt with some reason, as reported in the issue of the *Veterinary Record* of September 4th (page 817).

It is difficult to see to what other conclusion the King's Bench Division could come. If a veterinary inspector's certificate is not accepted as conclusive, the whole fabric of the administration of the Diseases of Animals Act breaks down.

What is extraordinary in the case mentioned is that Mr. Richard Jackson should ever have been subjected to the indignity of a police prosecution at all. It seems a case in which the police acted with excessive zeal, to use a mild term, in issuing a summons. The question whether scab did or did not exist on the occasion is not of great moment. The matter is one involving a principle with far wider issues. A veterinary inspector's certificate must have a legal status, but if such status is to mean that whenever disease, or what is supposed to be disease, is found amongst his stock, an owner must necessarily be dragged before the police court, the matter becomes a very grave one; for cases must occur, from time to time, all over the country where owners are quite unaware of the existence of a notifiable disease amongst their stock, at the time of exposure, and to prosecute in such a case is an abuse. Knowingly to expose an affected animal is an entirely different matter, but in the present case it was, according to press reports, never actually proved that the owner had knowledge of the existence of disease, and therefore he should not have been taken to court at all. Once court proceedings were instituted it was necessary, in order to obtain a conviction, to invoke the "conclusiveness" of a veterinary inspector's certificate. It appears, at present, that an attempt is being made to saddle the veterinary profession with the whole brunt of a dissatisfied public opinion, whereas the matter is purely a police affair. I am, yours faithfully, F. T. Harvey.

St. Columb,
5th September, 1926.

A Tuberculin Test Enquiry.

To the Editor of The Veterinary Record.

Sir,—If the matter is of any interest, might I ask for criticisms or similar occurrences from any of my colleagues in the profession in regard to the following? Being local Veterinary Inspector under the County Council it is my duty to inspect numerous cattle under the Tuberculosis Order. In the case for enquiry, *viz.*, three milch cows, the facts were:

Owner.—One of the largest stock owners and breeders in the district.

Examination.—Concerning two of the animals I had no hesitation in giving an opinion. The third was in normal condition (note) *pulse good and steady*; temperature 103°; no cough. I applied the test, using a perfectly clean hypodermic and single dose taken from ampule, as applied to the other two cows.

Reaction.—All three cows reacted.

	I.T.	12.	15.	18.	21.
Chart.—	103°.	103·2°.	104·2°.	103·3°.	103·1°.

The test completed, the forms were posted to the authorities on the Saturday night.

On the following Tuesday I was surprised to hear that the cow was dead.

On the Wednesday I visited the slaughterhouse for the *post-mortem* examination and was struck with the enormous size of the carcase—in every way similar to septic poisoning.

The carcase was split up as usual to get a good view of all organs and the first thing to attract my attention was the colour of the pericardium and the space involved. The former was black in colour and the whole took up half the thoracic cavity. On opening the pericardial sac, caseous material and liquid contents to the amount of about a gallon were exuded.

The heart itself was blue and atrophied. I did not make a more extensive examination of this organ as the surroundings were not very congenial to a too-lengthy stay. The lungs contained also tuberculous nodules. What caused the immense distension of the carcase? Did the action of the tuberculin cause premature death?—Yours faithfully, A. Scotson, M.R.C.V.S.

54 Queen Street,
Market Rasen,
Lincoln.
28th August, 1926.

The Editor acknowledges the receipt of the following:—

Communications from Dr. Juan E. Richelet, Messrs. F. T. Harvey and R. F. Montgomerie, Professor J. J. O'Connor and Lieut.-Col. T. Dunlop Young.

Report of a meeting of the North Midland Division, from Mr. J. S. Lloyd, Hon. Secretary.

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